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NERVE AND MUSCLE

BY MARION HINES

Department of Anatomy, Johns Hopkins University

IN THIS review of the present status of investigations upon the relation of nerve to muscle, the writer will consider only that of skeletal muscle. Exact knowledge of the termination of nerves on the smooth muscle and heart muscle fiber has progressed little in the last twenty-five or thirty years. Although both the parasympathetic and the sympathetic innervate these two types of muscle, nothing is known about the relation of these two divisions of the autonomic system to individual muscle elements (Woollard's (1926) excellent monograph on heart muscle contributes nothing to this particular aspect of the question). The only instance in which the particular endings of the autonomic system have been distinguished in their relations to individual cellular elements is the recent study of Stormont (1926) on the submaxillary gland of the rabbit. If this same type of work were to be extended to the innervation of other glands and in particular to heart muscle and smooth muscle, we should have an interesting anatomical basis for certain physiological peculiarities. Here, however, the questions of plurisegmental innervation, the type of fiber in skeletal muscle, the muscle spindle or sensory organs of muscle, the motor endings, and finally a discussion of the recent papers on the mooted question of tonus, particularly in its relation to the nerve endings, will be considered.

PLURISEGMENTAL INNERVATION

In an investigation of the muscles innervated by the *N. medianus* and *N. ulnaris* in domestic animals, Agduhr (1915) found that the sum of the contractions resulting from the maximal stimulation of each nerve separately as seen in *M. flexor digitorum sublimis* and the lateral portion of the humoral head of the *M. flexor digitorum profundus* was greater than the contraction obtained by the simultaneous maximal stimulation of both nerves together.

In the cat the *M. flexor digitorum sublimis* is innervated by the 7th and 8th cervical and the 1st thoracic nerves. Agduhr (1916) cut the 1st thoracic nerve 90 hours, and the 7th cervical 58 hours before killing the animal, allowing the 8th cervical to remain sound. He found

that the same muscle fiber might be innervated by both the 7th and the 8th cervical. It happened that in all muscle fibers having two motor end plates, one of them showed degeneration. In 1919 he cut the ventral roots in the cat at C_7, 96 hours and Th_1, 192 hours before sacrifice of the animal and examined the *M. interossei* with Bielschowsky's technique. In another animal C_7 was cut 96 hours, and C_8 168 hours, before the animal's death. In a few instances the same muscle fiber showed that it was innervated by C_7, C_8 and Th_1; but the majority were bisegmentally innervated. On the other hand, Floresco (1903) and Cavalié (1901) on purely morphological grounds have described motor fibers giving off terminal filaments to two or more motor end plates in the same muscle or to adjoining muscle fibers or muscle spindles.

Such a peculiar morphological finding as that of Agduhr could not remain unchallenged long. In 1924, Cattell and Stiles found that on stimulating separately each of the nerve roots to the frog's gastrocnemius the sum of tension developed in an isometric tetanus exceeded in the ratio of 1.7 to 1 the tension developed when the entire sciatic nerve was stimulated. They concluded that about 70 per cent of the fibers have double segmental innervation. Katz in the same year studied the heat developed by stimulating the two components of the sciatic nerve separately and together and found it to be the same, within the limits of experimental error. There is, then, in his estimation no plurisegmental innervation of frog's muscle. De Boer (1926) studied the action current of the same muscle in the frog and thought that the monophasic deflection was the same whether the *N. viii* and *N. ix* were stimulated separately or together.

Fulton (1925) studied the gastrocnemius of *Rana temporaria*, innervated by *N. viii* and *N. ix*, by measuring simultaneously the plateau tension and the electrical response. Although the result of a simultaneous stimulation in both cases was less than the sum of separate stimulations, and the difference varied in the same direction in both cases, nevertheless, the percentage of variation was two to three times greater in tension than in electrical response. Therefore, he believes that the amount of tension produced is not necessarily a real criterion for plurisegmental innervation.

Such a dilemma can only be resolved by an actual study of the muscle itself. We should know definitely about its innervation and then study its physiological behavior. As far as the writer knows, there are no morphological studies on the gastrocnemius muscle but from studying the sartorius after section of *N. ix* in the frog it is quite clear that many of its fibers are plurisegmentally innervated.

RED AND WHITE MUSCLE

In Cobb's (1925) recent consideration of the tonus of skeletal muscle and in Needham's (1926) review, the histology of red and white muscle is adequately discussed. However, as some understanding of the essential differences between these two types of muscle is necessary, a brief discussion of the salient observations will be given. Hunter (1925, p. 13) reopened this old question when he suggested "that the skeletal muscle of vertebrates consists of two sets of muscle fibers disposed in groups, each with its own specific innervation, and consequently, its own specific function." He believed that the fibers receiving the somatic innervation were similar to the "movement muscle" of invertebrates and that those innervated by the sympathetic nervous system resembled the fixing muscles in that they

remained at the new length imposed upon them. Such a broad generalization would be of fundamental importance were it true. As is evident it must not only be made to rest upon physiological evidence but its strongest prop is to be fashioned out of histological findings.

It does not take a trained scientist to discover that the meat of vertebrates varies in color; that of fish, amphibians and reptiles is more or less colorless while that of birds and mammals may vary from white to deep red. This difference in color was at first thought to be due to a greater blood supply found in some muscles than in others. But Kühne (1865) after carefully washing out the vessels of red muscles, prepared water extracts of the minced substance. He examined the absorption spectrum of such a preparation and thought the pigment to be hemoglobin. For further confirmation he prepared certain derivatives of hemoglobin such as hematin, CO-hemoglobin, and reduced hemoglobin from the extract. In 1871 Lankester published a paper upon the distribution of hemoglobin in the animal kingdom, paying particular attention to it as it is found in invertebrates. In some he found it as a free pigment in the body fluids and in one as a pigment of the head ganglion. In *Limnacus* and *Paladina* (1871) he found that the muscles of the pharynx contained a pigment which was spectroscopically identical with hemoglobin. He concluded in general that hemoglobin was found in those places where either the oxidation processes were rather rapid or had to be performed in the absence of an oxygen supply easy of access. Also it was found in those muscles where prolonged activity was required. Recently Keilin's (1925) studies on the properties of certain animal and plant pigments have substantiated this older work. For instance, he demonstrated that the red muscles of the guinea-pig show two bands of HbO_2 when exposed to air, which do not correspond exactly with those of HbO_2 from the blood of the same animal. In the pigeon the alpha band of HbO_2 of muscle is 5805, that of blood, 5763. However, these two pigments are as similar in the same animal as hemoglobin itself is when one species is compared with another. When the pectoral muscles of the pigeon are used for this study it is necessary to add a very dilute solution of KCN, in order to break up the oxidation system of the tissue—certainly a finding pregnant with implication for the physiology of red muscle.

The gross appearance of these two kinds of muscle is particularly striking when the white pectoral muscles of the domestic chicken are compared with the dark red of the goose, duck and pigeon or of birds which are relatively infatigable in flight like the buzzard or falcon (Starling, 1920). Although Meyer (1875) was able to confirm Ranvier's (1873) finding that the adductor of the rabbit was white and the semitendinosus was red, he found that such muscles as the *flexor digitorum communis* and the masseter which were red muscles had the histological characteristics of the white. There appears even after the exhaustive studies of Knoll a great difficulty in making any adequate generalization as to the distribution of red and white muscle. Starling (1920) is perhaps correct that all striated muscle of higher vertebrates contains both types of muscle fibers, and but few muscles are almost exclusively red or white. In man, where the distribution appears to be more mixed than elsewhere (Roberts, 1916), the muscle fibers of an athlete are not only larger in diameter than those of the ordinary man but also redder (Morpurgo, 1897). The minute histology of such a

change has not been studied as far as the writer is aware,—that is, we do not know whether the increase in redness is due only to the increase in size of the red fiber, with corresponding increase in the amount of pigment, or whether some of the white fibers also contain pigment. Were the latter true the morphological differentiation would not appear as great.

These two types of muscle fibers may be distinguished morphologically as well as spectroscopically. The amount of sarcoplasm is relatively greater in the red muscle fiber. Its nuclei are not always found immediately beneath the sarcolemma, as is true for the white muscle fiber, and the diameter of the red is much less than the white. Meyer (1875) described the overlying sarcolemma as brighter and the oval nuclei as lying in a finely granular sarcoplasm. Schaffer (1893) reported for man that the clear sarcoplasmic or white fibers contained small myofibrils arranged rather regularly, while in the granular sarcoplasm or red fibers, the myofibrils are large and the arrangement without order.

In 1883 Grützner published studies on the histology of these two fibers. He thought that the more granular fibers were similar to the red and that the less granular, or larger clear fibers corresponded to the white. A few years later Knoll spoke of them as protoplasm-poor and protoplasm-rich fibers. Such fibers, not distinguished by a content of pigment, make up the skeletal musculature of amphibians and reptiles. Grützner studied in particular those of the frog, where all are white in color except the superficial ones of the neck (and of course the heart). The smaller fibers, that is, those poor in protoplasm, contain granules which according to Knoll stain with the osmic acid of Flemming's fluid, and are reddened by the gold salt in the Ranvier technique. Krause suggested that the smaller of these two types of muscle fibers, together with a third, smaller in diameter than the other two and having its nuclei in the center, were stages in the development of the large fiber. Then Bonhöffer studied the distribution of these three types at various ages and found that they appeared in about the same numbers.

In recent unpublished studies upon the intercostal and neck muscles of the newborn sloth, Wislocki (1926) has observed that the myofibrils of all the fibers are exceptionally large and their bands broad and especially distinct. They are so loosely spaced in the sarcoplasm that the cell appears to be longitudinally striated. The nuclei are larger and found more often near the periphery of the fiber than its center. The sarcoplasm is not as granular as that which lies about the centrally placed nucleus of the granular fiber in the muscle of *Amblystoma*. Rather it is quite similar to the small fiber of the frog. In the recently dead adult, all the muscles of the limbs vary from red to deep red or to reddish brown. None of them appear white. The histology as far as known certainly classifies the muscles examined as red. In no other mammal so far studied has red muscle dominated.

The writer has studied certain muscles of the frog, the sartorius, the gastrocnemius and the *pectoralis abdominalis*. If these three muscles are watched in isotonic salt solution under oil immersion or high power, the first difference which appears between the three types of fibers is one of diameter and nuclear arrangement. As the study is continued the middle sized fiber becomes very granular; the longer it remains, the more granular it becomes. If, however, the muscles are fixed through the circulation with either Bensley's (1911) acetic-osmic-bichromate mixture

or with formalin Zenker, there is no difference in granulation. It is quite true, as Knoll observed, that these granules are reddened by gold chloride; but when the fixative is quick in its action after death, there are no granules which blacken in osmic acid. The very small fibers of Krause do not show these granulations. This is the fiber of the muscle spindle, the intrafusal fiber of Sherrington (1894).

If the muscle is allowed to stand an hour or more following the intravascular injection of methylene blue, the small fibers turn blue first, then the intrafusal fibers and sometimes the larger or clear fibers are slightly tinted. In cross sections of the whole leg of the frog, the diameter of the fibers of its individual muscles varies tremendously. The cross dimension of the large fibers in the sartorius is almost the same as that of the smaller ones in the gastrocnemius. It is quite clear that the distinction in size holds within an individual muscle but size alone cannot remain a criterion, when another muscle is considered. However, it is always the smaller fiber within a given muscle which grows granular upon death, has its nuclei scattered through the sarcoplasm and stains with methylene blue. Bonhöffer (1891) thought that the white muscle could reduce methylene blue faster than the red. Ahlgren (1921) found for the rabbit's chopped muscle that the reduction limit is much less for red muscle using the methylene blue technique. These two findings are not necessarily contradictory, for the cytological changes appear to take place sooner in the granular than in the clear muscle, not only the fiber but also its nuclei being colored. To the student of living tissues this is a criterion of death and subsequent permeability. The initial time of the reduction of methylene blue to the leuco-base may be shorter in the case of the red than the white and yet the red may die sooner.

Further, there is no difference in the distribution or size of the mitochondria or of the glycogen granules in the two types of muscle fiber in the frog. There was no extra-nuclear iron, as judged by Bensley's Prussian blue reaction, nor was any hemoglobin present as determined by MacCallum's method (1890). Keilin (1925) using another method, also found no hemoglobin in frog muscle. The cytoplasmic method (MacCallum, 1890) for the detection of phosphorus was not used. After Bensley's (1906) careful study of this technique, it appears almost worthless. Were the method more reliable it might be possible to detect a microchemical difference in the phosphorus content between the white fiber and the red. Gross chemical methods have demonstrated the white muscle to contain more than the red (Needham, 1926). If these methods could be applied microchemically it might aid in determining whether or not the clear fibers and the granular fibers of lower vertebrates were similar to the white and red fibers of birds and mammals.

Besides their differences in morphology and chemistry red and white muscles are distinguished by certain individual physiological peculiarities. Ranvier (1875) demonstrated for the first time that it took more stimulations per second to produce a continuous contraction in white muscle than in red. Later he found the length of twitch shorter in the white fiber, the latent period one-fourth as long, the tetanus much less, the fatigue greater, and the after-tetanus excitation much less than in the red fiber. This was followed by Grützner (1883) who showed that red muscle contracted more slowly and had a

longer latent period. Adrian (1925) says that

> the time-relations of the contraction are much slower in red muscle than in white, so that a red muscle fiber should be able to give a sustained contraction with a much smaller energy expenditure than a white. But the contractions are of essentially the same type in the two and the electric response of red muscle has the usual form, though it is slower in development and decline.

Cobb and Fulton (1925) confirmed the earlier work, namely, that the electrical response of white muscle is more rapid than red and that its latent period is shorter. In addition, they showed a prolonged flat top in the curve of the red muscle. They concluded, and rightly, that its contraction does not resemble smooth muscle. This prolonged contraction of the red muscle is beautifully demonstrated by the work of Hay (1901) in his comparison of the contractions of the semimembranosus with the soleus in the rabbit in response to a single break shock through the sciatic with and without curare. He found also as might be expected that ligature of the blood vessels affected pale muscle more than red. Reflex excitation by pinching the tibial nerve at the ankle elicitated a vigorous and rapid reflex contraction of the semimembranosus with little or no response from the soleus. After strychnine the former was exaggerated: the latter, irresponsive.

Contrary to Hay's results on the rabbit, Fischer (1908) in comparing the action of the gastrocnemius with the soleus in the cat found only a negligible difference in the latent period. The twitch of the soleus was higher than the gastrocnemius and, like Hay's finding, the time of contraction was longer. The curve for the isometric twitch is prolonged for the soleus, and is slightly higher than that for the gastrocnemius. A frequency of 80 stimuli per second is necessary for the gastrocnemius to attain a smooth tetanus of a height (54 mm.) comparable to that (53 mm.) which the soleus reaches with 13 per second; the time of development for the former is one second, for the latter, three; but strangely enough, the height for the white muscle at the beginning of tetanus is almost three times that of the red (40:15 mm.). In an isometric twitch the gastrocnemius reaches a tension of 600 gm., in isometric tetanus 700 gm. for a relatively short time; on the other hand, with the soleus the tension of isometric twitch is 200 gm., but with repeated stimuli it can maintain a tetanus of 950 gm. for a long time. It is interesting that the relative tension given by Hay after a stimulus of a single break shock was of the same ratio of 3 to 1, i.e., 150 gm. to 50 gm. for the semimembranosus and the soleus of the rabbit. In the other laboratory animals studied by Fischer, the rabbit, rat and guinea-pig, variations between the red and white muscles were in the same direction as those in the cat with one exception, namely, the soleus twitch was never so high as the gastrocnemius.

Roberts (1916) found, in the rabbit, that the red fiber degenerated if anything more rapidly following section of the nerve to the muscle,—a finding quite opposed to what would be expected from Hay's demonstration that the lack of blood supply affected the red less than the white. He was unable to compare the reaction of degeneration of these two types of muscle, partly because of the difficulty of isolating the soleus from the gastrocnemius and maintaining at the same time its blood supply, and partly because of the varied development and atrophy of the soleus. He believed that the anatomical relations of red muscles such as the soleus, the crureus and the deep head

of the triceps are not without significance. If they entered into contraction simultaneously, the elbow, knee and ankle would be in a position resembling decerebrate rigidity. Red muscles usually unite adjacent segments of the limb while white muscles often pass over one or more joints. In such cases he believed that fixation of one joint is carried out by the deeper lying red muscle. But in man, the red and white fibers are not separated in particular muscles but combined in every muscle. "It is common knowledge that man is capable of performing movements of graded intensity." Those two facts may be associated as Roberts believed or they may not. He thought that the red fibers play a prominent rôle in the maintenance of posture, that they perform slow and gradual movements, that they act synergically with white muscle in the fixation of one joint so that another may be moved.

There is no fundamental anatomical difference in the innervation of these two types of muscle in the rabbit. The motor end plates were the same size and constituted in the same manner. Hay found that the semimembranosus weighed 7.1 gm. and the number of nerve fibers to it were about 4187: the soleus weight was 1.28 gm. and the fibers numbered 787—a ratio by weight of 5.6 to 1 and by number 5.3 to 1. In the osmic acid preparations the white muscle nerves varied from 15 to 18μ, the red from 10 to 13.5μ. He could discover no difference in the muscle spindles. After section of the 7th, 8th and 9th posterior thoracic roots the ratio of undegenerated nerves remained about 5 to 1. The only difference which appeared was in size of the nerve fiber and the thickness of the myelin sheath. Very recently, Feldberg (1926) has studied the auricular muscles of the rabbit after monolateral sympathetic denervation. No difference was noted between large and small fibers on the two sides.

It is rather striking that the color of skeletal muscle in birds and mammals with few exceptions may be correlated with the histological structure of the fiber itself; and perhaps still more so that this minute anatomical differentiation of muscles predominately either red or white should parallel difference in their physiological behavior. A physiological study of these exceptional muscles like the *flexor digitorum communis* and the masseter of the rabbit, which Ranvier found to be red in color but similar to the white in structure, might lend a suggestion for the function of the hemoglobin content of the red muscle fiber. By means of such an investigation, negative though it must be, some concept of the physiology of the large sarcostyle and its surrounding granular sarcoplasm might be reached.

The comparative study of the structure of muscle fibers of vertebrates other than birds and mammals has not demonstrated any particular muscle as composed more or less completely of either granular or non-granular fibers. Consequently, the physiology of such nerve muscle preparations as that of the gastrocnemius of the frog may not be compared with any justification to that of either red or white muscles in mammals or birds. The investigator is dealing with a muscle as mixed in type as the majority of those of higher vertebrates. So long as no differences in innervation of red and white fibers or of the granular and non-granular can be discerned with our present technique, the cause for their dissimilarity may be considered to lie in the structure of the fiber itself. This situation is further complicated by a finding in the frog (anticipating data presented on the motor innervation) that the same axis cylinder may divide and end hypolem-

mally in motor end plates of similar morphology upon a granular and a non-granular fiber. If for the pleasure of discussion the results of the study of the physiology of white and red fibers of mammalian forms be transferred respectively to the non-granular and granular fibers of certain reptiles and amphibians, a new difficulty raises its head, namely, the same nervous impulse must be translated by the two kinds of muscle fiber into types of contraction signally different, and the quality of activity of these two muscles would then be a function of their structure alone.

MUSCLE SPINDLES

The term "muscle spindle" is a translation of *Muskelspindel*, first used by Kühne (1863) for the small muscle fibers, spindle-like in form, found more or less encapsulated in the connective tissue septa of the skeletal muscles of higher vertebrates. In all forms they are shorter than the muscle fibers which make up the major part of the muscles. The separation of these elements from the rest of the muscle tissue varies greatly. In mammals, the intrafusal fibers are surrounded by a perilymphatic space, which Sherrington pleased to call periaxial, limited by a thick connective tissue capsule. No such space is found in birds or reptiles or tailless amphibians. Muscle spindles have not been found by the older workers (Kerschner, 1888; Giacomini, 1898; Baum, 1899) in fish nor in the tailed amphibians (Franque, 1890; Giacomini, 1898). As far as the writer knows, no work has been done since that time. Although there may be no spindles as such it is highly probable that some sensory mechanism exists. But what, is not known.

The interest in this subject was renewed by the work of Kulchitsky (1924) on the muscle spindles of the python (Plate 1, fig. 1). The writer wishes to give a brief résumé of the relation of the nerves to these structures as far as they are known at the present time beginning with amphibians. We shall consider the work on reptiles in detail, comparing their structure with that of birds and mammals. There is quite a variation, which may account for the difference in the physiological results reported for removal of different parts of the nervous system. These data with those which will be presented for other innervation of the skeletal muscle argue for a certain restraint in the presentation of generalizations for all vertebrates, resting upon morphological findings in the muscle innervation of one form.

The muscle spindles of the frog, or fascicles of Weismann, contain a variable number of muscle fibers grouped together. Franque (1890) described an insignificant connective tissue capsule lying about the area in the long fiber (the embryonic fiber of Krause) where the medullated nerve enters. Kölliker (1889) thought their diameter varied from 3 to 15μ. Cajal (1888) showed no striations at the elevation of the medullated ending, Huber and de Witt (1898) pictured the same type of medullated ending but showed striations in the region of the ending itself, which they believed to be epilemmal. Dogiel's description differed in no way from that of Huber and de Witt; like these workers he found that the medullated fibers leave their myelin and undergo frequent branching. None of these authors mentioned Cajal's description of the frog in which he said that a typical motor ending was found on the intrafusal fiber, at one side or sometimes on both sides of the sensory ending. This was substantiated also by Sihler (1900).

The writer finds that the ending described by the previous authors is indeed

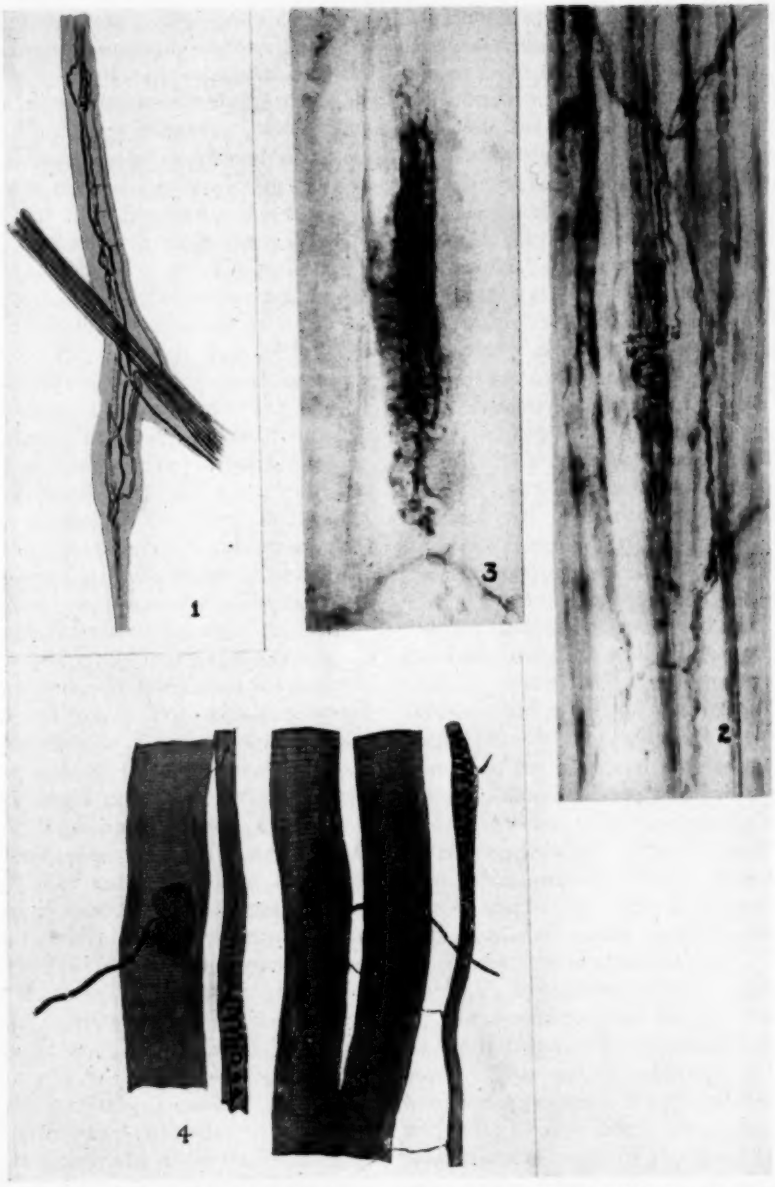

FIG. 1. Muscle spindle of the python, showing the ending of the medullated (thick) fiber, thought to be somatic sensory, and the non-medullated (thin) which Kulchitsky interpreted as sympathetic. Kulchitsky, 1914, pl. iv, fig. 11.

FIG. 2. Normal muscle spindle, stained with methylene blue, taken from the right *M. pectoralis abdominalis* of the frog (No. 171). The bulb-like processes are expansions of the axis cylinder of the somatic sensory fiber.

FIG. 3. Muscle spindle, stained with methylene blue, taken from the left *M. pectoralis abdominalis* of frog (No. 171) 47 hours after *N. iii* was cut at its point of exit from the spinal canal. The sympathetic was left intact.

FIG. 4. Muscle fibers of the lizard showing a motor end plate from which arises a fiber ending as a *terminaison en grappe* on the muscle spindle. From Perroncito, 1901, figs. 1 and 2.

sensory, because it disappears when the dorsal root ganglion is destroyed, as may be seen on subsequent examination of the sartorius with methylene blue (see Plate 1, figs. 2 and 3). Moreover, besides this ending there is another arising from a non-medullated nerve and appearing to fuse with it. And frequently the route of entry of such a nerve is the connective tissue sheath of a small blood vessel. The non-medullated fiber disappears when the abdominal sympathetic chain is removed. Peculiar as it may seem, when either nerve is destroyed, the clear picture of the morphology of the ending itself is disturbed, although the fibers of the other nerve entering the end organ are intact. A medullated motor end plate was never found on the spindle fiber. The writer, therefore, was unable to substantiate the finding of Cajal and Sihler, although the sartorius and *pectoralis abdominalis* of some two hundred frogs were studied.

It is interesting that Kulchitsky (1924) did not mention spindles in his study of nerve endings in frog muscle although he described at length what he called sensory endings. The writer found in the initial stages of this investigation leaf-like endings similar to his description, but later, when the technique was perfected, they were seen to be the knob-like projections of the sensory innervation of the spindle. The complete innervation of the spindle is so difficult of preservation, that the preparation must be studied within a short time, otherwise its brilliancy is lost. That is probably the reason why the earlier workers failed to find the sympathetic ending. At present the writer has no explanation for failure to corroborate the motor innervation by myelinated nerves as described by Cajal and Sihler.

The muscle spindles of snakes and lizards consist of only one intrafusal fiber, while those of the vastus muscles of the tortoise may contain as many as two to eight (Huber and de Witt). At the sensory nerve eminence many nuclei are found, but there is no interruption in the cross striation. There is no proof in the literature on the spindles of reptiles that the so-called sensory termination is such in reality, for the dorsal root distal to the spinal ganglion has not been cut nor has the ganglion itself been destroyed. Besides this "sensory" innervation, Ranvier (1878) pictured another which he interpreted also as sensory, lying outside the connective tissue capsule. This second ending was first considered by Cajal (1888) and Kerschner (1888) as motor. Later this was believed to be demonstrated by the morphological studies of Sihler (1900), Cipollone (1898) and Perroncito (1901 and 1902, Plate 1, fig. 4), although Giacomini (1898) thought that both might be sensory. By the Loewot-Ruffini modification of gold chloride technique, Perroncito (1901) demonstrated a spiral ending about the central point of the spindle, while beyond that area, the intrafusal fiber received a non-myelinated ending, a branch from a medullated fiber. The termination of the other twig made a typical motor end plate upon striated muscle. Sometimes this second ending took its origin from the medullated motor fiber immediately proximal to the motor plate itself. Such a definite appearance of a divided axis cylinder would point to the fact that the second ending on the intrafusal fiber was motor. This ending, although it may have the appearance of a plaque (Kerschner, 1892), may also look like the *terminaisons en grappe* of Tschiriew (1879) (see Sihler, 1896). Nevertheless, Giacomini (1898) considered it to be sensory because of its morphology. Huber and de Witt (1898), however, although they

pictured these endings as epilemmal, regarded them as originating from a non-medullated fiber and described nothing which resembled Perroncito's findings. Besides this non-medullated motor plaque Perroncito (1902) using the gold chloride technique pictured fine non-medullated fibers twisting about the sensory innervation of the spindle. In 1924 Kulchitsky (Plate 1, fig. 1) using methylene blue technique described in the python the single intrafusal fibers of the spindle, innervated by a non-medullated nerve fiber. This fiber he believed to be sympathetic, and the ending from the medullated fiber to be somatic sensory. He worked with teased specimens and fixed with a combined ammonium molybdate and picrate solution. At no time did he report finding the "sympathetic" innervation arising as a branch from the somatic motor innervation of the skeletal musculature. Perroncito, Huber and de Witt, and Kulchitsky all agree in the fact that this fiber near its point of entrance into the spindle is non-medullated; the first two describe it as an ending similar in morphology to the motor end plate, while Kulchitsky with Tschiriew called it a grape-like termination and Bremer (1883), umbiliform. No one has found the very fine non-medullated fibers described by Perroncito in his last paper.

If Perroncito is correct, we find an interesting difference in the motor innervation of the intrafusal fibers of reptiles and amphibians. The former are innervated by a somatic fiber and the latter by a sympathetic. If, however, Kulchitsky's guess is correct, their motor innervations are from the same system. It is difficult to discount such a definite finding as Perroncito presents. Also if Perroncito is correct it is more difficult to understand Sherrington's (1894) finding that the fiber did not show any morphological sign of degeneration following section of the ventral roots. It must also be remembered that the intrafusal fiber of reptiles is very similar to red muscle and is regarded by Sherrington as red in higher mammals. Under these circumstances two types of muscle according to Perroncito would be innervated by the same somatic fiber. This discrepancy may be due to fact, the muscle used for study, the difference in technique, or the particular animal used. Until it is repeated with some accompanying degeneration studies, even the simple anatomical relationship may not be known.

In birds the number of intrafusal fibers varies from two to six in the spindles (Huber and de Witt for the pigeon) within the same muscle. The cross striations of the intrafusal fiber are in evidence even at the nerve eminence (Cipollone, 1897) although less marked than at either end of the muscle. The medullated or sensory fiber enters the sheath of Henle and is distributed over the intrafusal fiber in a manner resembling the description for reptiles. Accompanying it is a non-medullated fiber ending in what Giacomini (1898) called *terminaisons en grappe*, Cipollone (1897) a motor plaque, while Huber and de Witt (1898) do not appear to have seen the fiber in question, although they found a non-medullated fiber within the capsule. They called it a branch of the sympathetic but did not assign any function in relation to the muscle spindle fiber itself. If this nerve were proved to arise from the sympathetic motor cells, and its function varied a little from that found in mammals, there might be a possible morphological basis in the claims of Hunter and Royle for the loss of tone following removal of the sympathetic in the wing of chickens and sea gulls. At present, however, almost nothing definite is known about the innerva-

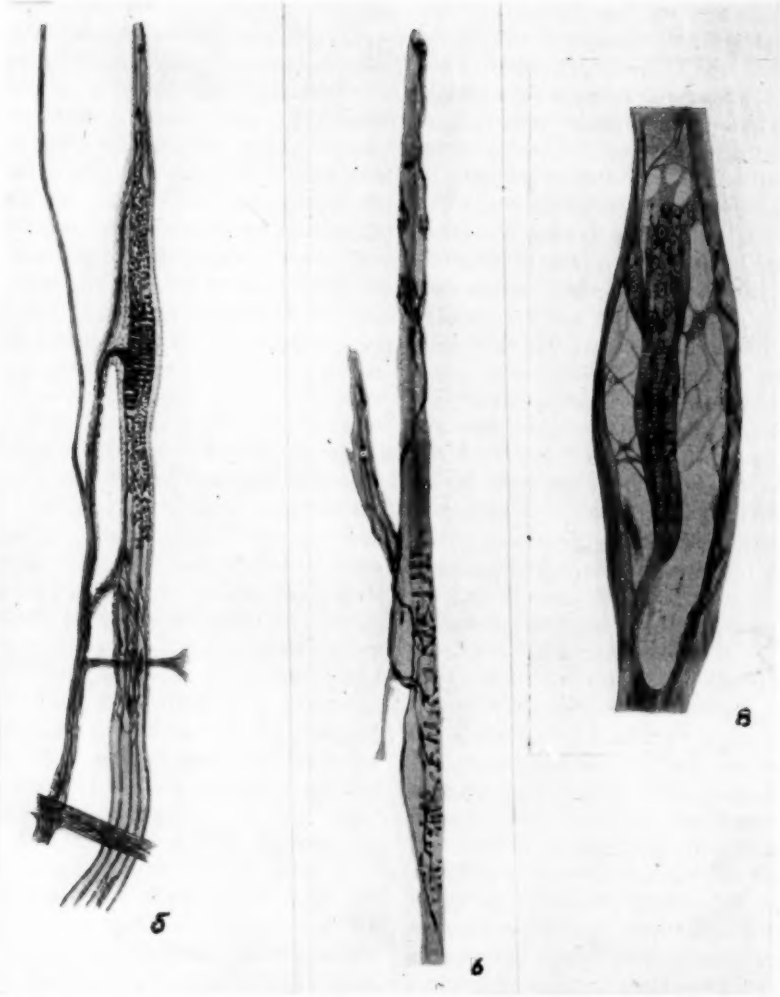

FIG. 5. Neuro-muscular spindles from adult cat, showing a plate-like ending, similar to the *terminaisons en grappe*, the central primary spiral and the secondary flower-like ending of Ruffini. Taken from Plate ii, fig. 1, 1898.

FIG. 6. Methylene blue preparation of a muscle spindle from the abdominal muscles of the rabbit, showing the spiral ending of Ruffini and the *terminaisons en grappe* from a non-medullated fiber. From Dogiel, 1902, Taf. II, fig. 14, iv, a.

FIG. 8. Muscle spindle from the *M. interosseus digiti* in the cat, stained by the Bielschowsky silver impregnation technique. The degenerated ending is that of C_7. The intact ending arose from T_1. From Agduhr, 1919, fig. 6.

tion of the muscle spindle in birds. It is always possible that a slight difference in morphology may exist and if so, a modification of function might accompany it.

The muscle spindles of mammals are rather isolated organs lying generally in the connective tissue septa between the small fascicles of skeletal muscle fibers. From three to six intrafusal fibers are enclosed in a thick connective tissue capsule (Huber and de Witt, 1898). The capsule contains a blood supply of its own. The intrafusal fibers are surrounded by the periaxial lymph space which Sherrington was able to inject through the lymphatic system. Sympathetic nerves are seen entering it, to do service on the vessels (Huber and de Witt). The spindles vary greatly in length and diameter, according to their age, the particular animal studied, and the muscle in which they are found. Baum (1899) gives for adult man 2 to 10 mm. in length, 0.08 to 0.25 mm. in width; Sherrington (1894) for the cat and ape, 0.75 to 4 mm. and 0.080 to 0.25 mm.; and Batten (1897) for the dog, 3 to 4 mm. for their length and 0.16 mm. for width. The fibers themselves in adult man are about 0.0135 mm. in width as compared with ordinary muscle fibers of 0.0315 mm. (Christomanos and Strössner, 1891). In the mammals studied by Huber and de Witt (1898) spindles were found with areas of nerve distribution varying from one to four. The former were most numerous in the guinea-pig and rat, while the latter were found more usually in the larger laboratory mammals, the dog, cat and rabbit.

Generally speaking, the spindles are more numerous in muscles of the limbs than in those of the trunk, the head or the neck. Onanoff (1890) found them very abundant in the intrinsic muscles of the hand, the flexors of the fingers and the quadriceps femoris. It will be remembered that Huber and de Witt studied the intrinsic plantar muscles of the dog. They are more numerous in the muscles of the thorax than in those of the abdomen. Fraenkel (1878) found them particularly numerous in the thenar eminence in man. In certain muscles, however, Sherrington (1897) says that they are entirely lacking, namely, the orbital eye muscles, the intrinsic muscles of the larynx, the intrinsic muscles of the tongue and the diaphragm. On the contrary, Sutton (1918) was able to demonstrate the neuro-muscular spindle in the extrinsic eye muscles of the pig by following the development of nerve and muscle contacts. Pilliet (1890) and Langworthy (1924) have found them in the intrinsic muscles of the tongue in the cat. Cipollone (1897) has not found them in the muscles of the face in the rabbit, nor has O. Cushing Smith (1926, private communication) found them in the facial muscles of the fetal pig, the cat, the guinea-pig or the rat. Cavalié (1902) has not found them in the cremasteric muscle. Baum (1899) did not see them in the muscles of the external ear, in the *M. laryngo-pharyngeus*, the two bellies of the digastric, the stylo-hyoid, or in the muscles ischio-cavernosus and the bulbo-cavernosus. Cipollone (1897) has found them in the external pterygoid and the masseter muscles in the rabbit, while Cuajunco (1926) has demonstrated them in the latter muscle of the fetal pig.

The sensory terminations on the muscle spindle of mammals were seen for the first time by Kerschner (1888) in man. The myelinated fiber entered the capsule, lost its myelin sheath and wound itself about the intrafusal fiber. In the next year Koelliker demonstrated a spiral-like termination. But the best morphological work was that of Ruffini (1892) upon the cat (Plate 2, fig. 5), with a few accessory

studies upon the spindles in man. Using the gold chloride technique he described three different endings, primary terminations or annular spiral, secondary terminations in the form of flowers, and motor terminations resembling motor end plates. The spiral endings of Ruffini are considered by Huber and de Witt as the most typical. They were also described again by Kerschner (1892).

The *terminaisons en forme de fleur* of Ruffini, according to Kerschner and Huber and de Witt are terminal endings of the spirals or their branches, and do not belong in a separate category. In the rat, guinea-pig and rabbit, these spiral endings are not as apparent as they are in the cat, dog and man. Dogiel (1902) working with spindles from the *M. transversus abdominalis* of the rabbit described spiral endings which arise from heavily medullated fibers and grape-like terminations from thinly myelinated fibers. Ruffini thought the primary ending had a heavier medullated sheath. Huber and de Witt, however, described the secondary sensory endings as branches from terminal non-medullated continuations of the spindle nerve which have a zigzag course in an intrafusal fiber without forming a spiral. Both Kerschner and Ruffini have studied the spindles in man and say that the annulo-spiral endings are not there. The termination is compact, and, from their description, somewhat resembles that found in the rabbit.

Kerschner and Ruffini described what they thought was a motor ending. It was probably the same as Dogiel's *terminaison en grappe* (Plate 2, fig. 6), which he believed to be motor. Huber and de Witt were inclined to agree with Kerschner, that some at least of the intrafusal fibers have motor endings. They showed some small plaques which were similar to the pictures of the other three workers.

In rare cases Huber and de Witt said that they found sympathetic nerve fibers in the capsule of the spindle. They believed them to be vaso-motor fibers for the blood-vessels of the spindle muscle and to have nothing whatever to do with innervating the intrafusal fiber itself. Dogiel also saw such fibers and thought that they belonged to the sympathetic innervation of the blood vessels.

These endings described as motor, the *terminaison en grappe* of Tschiriew (1879) at first were thought by him to be sensory; later, although he found them upon fibers with a well developed motor end plate, he continued to interpret them as embryonic forms of motor plaques. Huber (1899) found them upon the extrinsic muscles of the eye of the rabbit. Because there were other typical motor endings, because they formed no Doyère's elevation, were extra-sarcolemmic, were not found in the *retractor bulbi*, were more numerous in the anterior third than the middle third of the muscle, and because no other sensory endings were found in these muscles, he thought them to be sensory. The last reason for believing them sensory vanishes if Sutton's (1915) description of the spindle in the pig proves to be more than a particularization. In the same year (1899) and a year earlier, Sherrington studied the question of the sensory innervation of the eye muscles in the cat and monkey. He reported only peculiar branching of nerves about the tendons, or at the tendon end of the muscles; these nerve branches were undisturbed by section of the ophthalmic division of the trigeminus and were only destroyed by cutting the motor nerve to the eye muscles close to the brain. Elliot Smith (1926) in a discussion at the proceedings of the Section of Neurology, said that Woollard had found such an ending in the eye muscles of the cat and

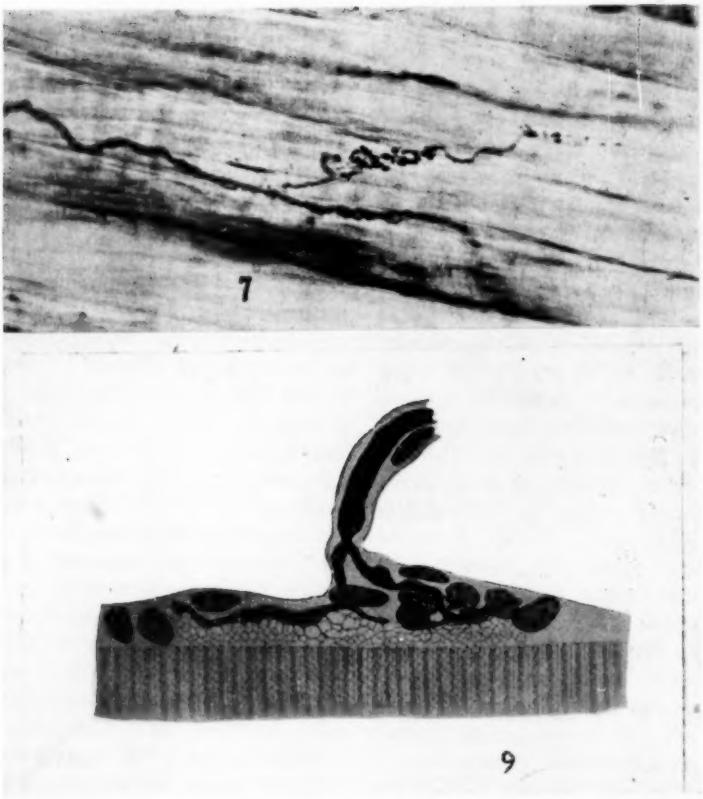

FIG. 7. *Terminaisons en grappe* stained with methylene blue, found in the *M. pectoralis abdominalis* in the frog. These endings are epilemmal.

FIG. 9. Normal motor end plate on a muscle fiber of the cat's *M. obliquus superior* seen "in profile." The neurofibrilar network ends in the periterminal network, which Boeke believes to be the morphological framework of Langley's neuromuscular junction. Bielschowsky silver technique. From Boeke, 1921, fig. 7.

rabbit. Latham (1924) studied muscles from the limb, the intercostals, the extrinsic (*levator palpebrae*), and the intrinsic muscles of the eye of the fowl, the sea gull and goat. He came to the conclusion that the slender muscle fibers were provided with endings which resembled the *terminaisons en grappe* of Kulchitsky's (1924) description for the small muscles of the python. However, in reptiles (Perroncito, 1902; Kulchitsky, 1924) and in amphibians such as the frog (Plate 3, fig. 7) these terminations or the umbiliform endings of Bremer (1883) are found, especially in the tongue, and in such muscles as the *pectoralis abdominalis*, and the submaxillaris of the frog (Hines, 1926). Landauer (1892) found that they disappeared in the frog when the anterior roots were cut. Although it is not possible to reason from morphology as to whether the ending is sensory or motor, it is not beyond possibility that the *terminaisons en grappe* are sensory in the eye muscles and motor in the spindles and wherever they are found upon skeletal muscle fibers. Moreover, in the frog they do not necessarily appear only in connection with the small fibers, nor do they take the place of a well defined motor end plate as Kulchitsky described for the python (1924), but did not mention for the frog (1924).

In mammals, besides these peculiar endings which are believed by certain authors to be sensory, there are terminal interstitial ones in the intercostal muscles of the dog (Dogiel, 1903) and unencapsulated endings in the abdominal muscles of the rabbit (Dogiel, 1902). Sherrington (1897) said that Pacinian corpuscles occur in the intrinsic muscles of the larynx. Horsley has found them in the gastrocnemius muscle of the cat. Timofejew (1902) has described them in the diaphragm of the guinea-pig and rabbit. Krause (1876) saw them in the flexors of the digits in the sheep. Rauber (1882) found them on the surface of the pectoral muscles of birds. Although it is not absolutely true, nevertheless we may say in general that in those muscles where spindles are not present some other type of ending may be found which investigators believe to be responsible for the afferent impulses leaving the muscles and which probably allows for a better peripheral control of such organs by the central nervous system.

Now, what proof have we of the origin of fibers innervating the muscle spindle in mammals?

Sherrington (1894) cut the ventral roots and the dorsal roots between the spinal cord and the dorsal root ganglion in the cat and monkey. He then obtained a degeneration of the motor fibers originating from the cord, with the sensory nerve intact, but incapable of function. Under that condition the heavily medullated fiber to the muscle spindle appeared normal, 30 to 190 days after the operation. In the cat he cut the sciatic nerve and killed the animal four months later. All the muscles were atrophied except the intrafusal fibers of the spindle which remained intact in spite of the fact that he could distinguish no nerve fibers. He concluded that the intrafusal muscle fibers were not dependent upon the ordinary motor nerves for their maintenance. Nevertheless, his results suggest that the sympathetic, which remained in both instances, was reponsible for the integrity of the intrafusal fiber. Onanoff (1890) attempted to destroy the spinal ganglion, but did not feel sure that the destruction had not injured the ventral root also. In this experiment he found sometimes a few nerve fibers of small calibre in the neuromuscular spindle.

Cipollone (1898) compressed the ab-

dominal aorta temporarily in the rabbit and produced a necrosis of the grey matter in the cord with a subsequent paraplegia, flaccid in type. When he examined the spindles four to ten days later, he found the large medullated fibers intact but the finely medullated fibers and their plate-like terminations on the intrafusal fibers degenerated. If this be true, the plaque is motor, that is, its cell body lies in the ventral horn of the cord, but does not control the nutrition of the spindle. However, the rationale of the experiment is not sufficiently clear cut to be decisive.

Morpurgo (1897) found that exercise in no way modified the size of the individual intrafusal fiber although he found a marked hypertrophy of the skeletal muscle itself. If these nerves are motor they do not affect the intrafusal fiber as do the motor nerves to ordinary skeletal muscle fibers.

Tello (1907) observed that after section of the nerve, the two forms of nerve terminations in the fascicle of Weismann—that is, the termination of the fascicle of Kühne and the motor plaque,—degenerated at the same time, but not with the same rapidity. In preparations 18 hours after section, where the plaques were found in the first phase of degeneration, the fascicle had reached the third or fourth phase. But this does not adequately answer the question of their several functions, although it demonstrates that they are not alike physiologically any more than they are morphologically.

In the same research in which Agduhr (1919a) reported the plurisegmental innervation of skeletal muscle, he presented a similar study upon the muscle spindle. In the cat he cut, in the immediate neighborhood of the *foramina intervertebralia*, the 7th cervical nerve 58 hours, and the 8th cervical 144 hours before death. Afterwards he checked by autopsy and stained with the Bielchowsky silver impregnation method. In the *M. interosseus digiti II* of the anterior extremity, he showed one part of the sensory ending as completely degenerated, C_7, and another as completely intact, Th_1 (Plate 2, fig. 8). There was a slender non-medullated fiber lying in the connective tissue capsule, which he believed to be sympathetic in origin. He thought that the ending upon the intrafusal fiber was sensory because it lay near the middle of the spindle organ and was epilemmal. Furthermore, theoretically, this technique should show a neurofibrillar structure if the ending were motor. Besides these innervated by C_7 and Th_1, he found another group of spindles innervated by C_8 and Th_1. In no case did he find one innervated trisegmentally. The extensor muscle of the forearm contained a smaller number of plurisegmentally innervated spindles than the antibrachial flexor muscles of the same animal.

In amphibians (the frog), then, the mass of data indicates that the motor innervation is sympathetic, that is, its cell body lies in the abdominal chain. In the work previous to Kulchitsky's (1924), leaving out that of Huber and de Witt (1898), the spindle was described as innervated by a non-medullated fiber, a branch of a medullated fiber. The authors mentioned do not commit themselves as to the origin of the non-medullated fiber. Kulchitsky, however, did say definitely that he believed it to be sympathetic. In birds we have nothing particularly conclusive. In mammals, the work of Onanoff and Cipollone suggests that the ending is somatic motor, while that of Tello and Agduhr shows definitely that it is somatic, but does not distinguish beyond doubt the motor origin of the finer medullated fibers.

And yet this nerve fiber must have been destroyed in Sherrington's (1894) study in which he reported a normal intrafusal fiber after the spindle was theoretically stripped of all its innervation capable of function, except the sympathetic. On the other hand, Agduhr pictures, but does not describe, what appears to be degeneration of the intrafusal fiber after cutting the somatic nerve distal to the ganglia. Functionally, Agduhr's experiments and Sherrington's section of the ventral and dorsal roots are equivalent; but morphologically, in those of the former the cell body of the sensory innervation was removed. The exact relation of the components of the peripheral nerves to the spindle and its contents is yet to be analyzed. The origin of these innervations is important for the further consideration of the part played by the proprioceptive fibers, i.e., the muscle sensory fibers, in the reflexes which maintain that elusive quality of muscle Bayliss pleased to call tone.

THE NERVE ENDINGS ON STRIATED MUSCLE

Doyère (1840) was the first to describe the relation of the end branches of nerves to muscle fibers. He noted that at the point where they came in contact a small hillock was visible, Doyère's eminence. At the base of this eminence a great number of nuclei, a part of the muscle, was noted by Kühne (1862). Later (1887) using for the most part the gold chloride technique, Kühne described the continuation of the connective tissue sheath with the sarcolemma, and the branching of the axis cylinder (*das Geweih*) which appeared to become confluent with the sarcoplasm of the muscle fiber; the sarcoplasm at this point was very granular, and surrounded by large transparent nuclei, (*die Sohle*), the sole. His demonstration by means of cross sections, that this ending was hypolemmal has been substantiated by other methods, such as Huber and de Witt's use of the methylene blue technique or lately by Boeke's use of the Bielchowsky silver impregnation method. Kühne believed that the sole is muscle protoplasm and its nuclei similar to muscle nuclei. Huber and de Witt (1898) found nothing granular in their methylene blue preparation of the rabbit motor end plates and suggested that these granulations may be due to the technique used. Kühne thought that the many branched endings in frog muscle were also hypolemmal, but Krause and Koelliker described them as epilemmal. At the termination there is no great heaping up of sarcoplasm, and only a slight increase in nuclei of the muscle itself. The morphology of this ending is more or less peculiar to each animal group. Indeed many times a particular animal might be identified by the morphology of that ending only. Boeke (1921) with a perfection of the Bielchowsky technique that wins the admiration of all who have attempted to duplicate his preparations, was able to demonstrate that the neurofibrils of the axis cylinder, probably a precipitation product, ramify in the form of a network in the motor end plate. This terminal network (Plate 3, fig. 9) of the axis cylinder may be demonstrated in cross section to be continuous with a similar network in the muscle staining brownish, the "periterminal network." Further, the periterminal network appears to be in protoplasmic continuity with the anisotropic bands of the muscle fibril. In only one case, that of *Amphioxus*, did he find the neurofibrillar structure to be continuous with the myofibrillar. The extremely delicate terminal meshes of this network were continuous with the anisotropic discs of the muscle plates themselves. After a

motor nerve has been cut this neurofibrillar network first swells and then fuses, and after two or three days breaks up and disappears; then the periterminal network follows and the sarcoplasm of the sole appears granular. In regeneration, shortly after the new nerve fiber has pierced the sarcolemma the periterminal network reappears in the neighborhood of the neurofibrils. Later it spreads out. The morphology of this ending appears to be closely related to the integrity of the nerve fiber innervating it.

It was early noticed that the nerve fiber was medullated but lost its myelin sheath in the vicinity of the point where it penetrated the sarcolemma of the muscle. This nerve is the motor nerve, the one which, when stimulated, causes the muscle to contract. The question is: is this fiber ever aided in that innervation by another of the same class or of another class? Bremer (1882) shows in figure 22 a motor end plate of a striated muscle from the linguales of *Lacerta muralis* which received a large medullated fiber and a small unmedullated fiber. In 1901 Perroncito substantiated Ruffini's (1894) finding that in reptiles, the lizard in particular, the medullated fiber is often found dividing into two branches, one medullated, the other, non-medullated. The former innervates a skeletal muscle fiber and the latter, the muscle spindle. And occasionally the non-medullated branch may innervate another striated muscle fiber, sometimes the large clear type and sometimes the small granular variety. Strangely enough this second nerve fiber, Ruffini's ultraterminal fiber, does not terminate in an end organ like the other branch, but in one which resembles rather the *terminaisons en grappe* of Tschiriew's description (Plate 4, fig. 10). Such a situation although many times more complicated may be seen in figure 14 of Bremer (1882), a preparation of the *musculus hyoglossus* of the frog. Bremer (1883) in figure 16 shows a non-medullated nerve coming off as a branch immediately before the motor ramification and innervating a spindle from the muscle of the tail of *Lacerta muralis*. It is possible then that the non-medullated fiber which entered the motor end plate may have been a converted medullated fiber of somatic origin. Indeed, the writer has often observed the same peculiarity in the sartorius of the frog, and has proved these fibers to be of cerebro-spinal origin for they persist long after all the sympathetic fibers upon the vessels have degenerated following complete removal of the abdominal sympathetic chain.

Grabower (1902) found in human muscle non-medullated and medullated fibers in the same sheath of Henle; for the most part they ended independently. The non-medullated fiber was often a continuation of a medullated one. Later Gemilli's (1905) observations tended to establish further that this non-medullated fiber was ultraterminal in the sense of Ruffini and Perroncito. A year later Botezat (1906) described in birds an innervation of skeletal muscle by both types of fibers. He did not think that the heavily myelinated fiber which forms the motor end plate became thin and non-myelinated. In fact the endings of the latter were often seen on the muscle fiber in the vicinity of the motor end plates. This second form of nerve ending in the muscle of birds and higher animals may not necessarily be similar to the form seen in reptiles.

From this work of Botezat we have the tempting suggestion that perhaps there are two types of fibers, having different origins. The first definite suggestion came from the work of Boeke (1909, see Plate 4, fig. 11, this paper). In a study

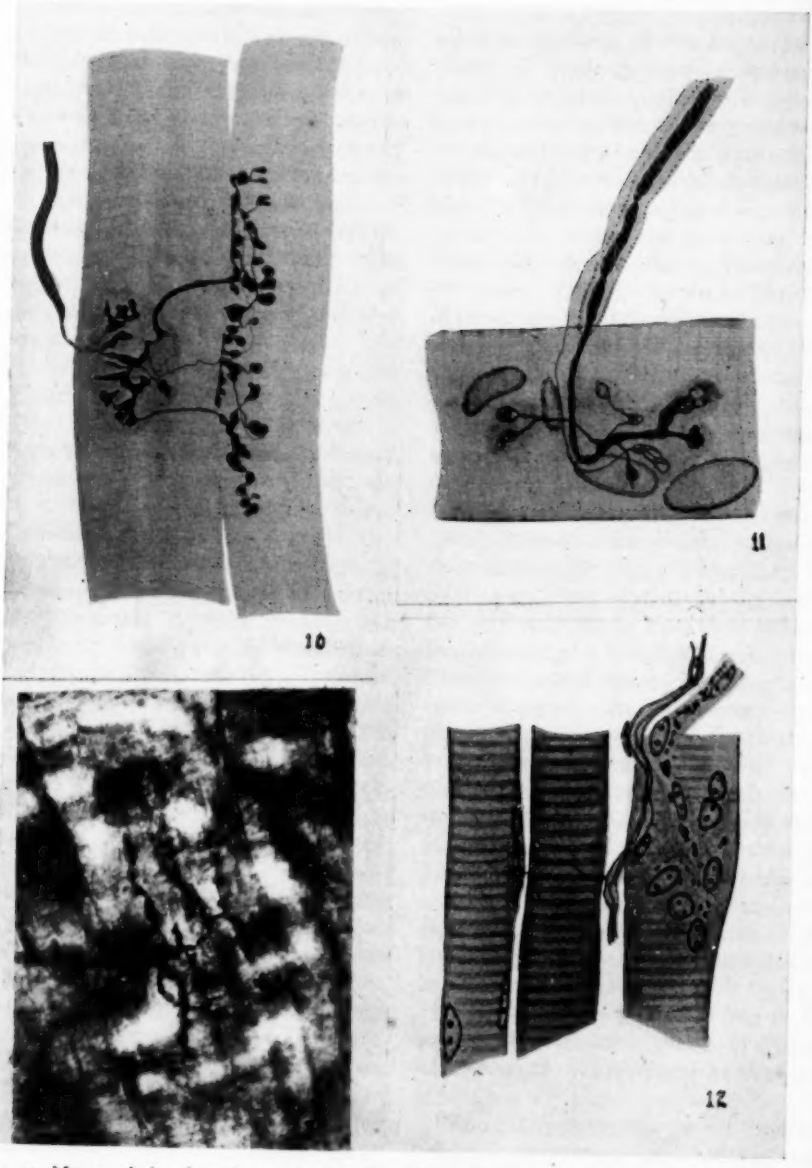

FIG. 10. Motor end plate from the muscle of reptiles. Nerve filaments from the motor plaque on the large fiber, which gives rise to a *terminaison en grappe* on a small fiber. From Perroncito, 1902, Pl. 2, fig. 11.

FIG. 11. Motor end plate on a muscle fiber from the back muscles of a lizard, showing the ending of the medullated motor fiber and of the accessory non-medullated fiber. Boeke, 1910, Taf. 1, fig. 24.

FIG. 12. Muscle fibers from the eye muscles of the cat. The medullated fiber ending in the plate has degenerated following section of the *N. trochlearis*. A non-medullated fiber has persisted. Boeke, 1913, fig. 10.

FIG. 13. Motor endings in the *M. sartorius* of the frog. The same axis cylinder innervates a large and a small muscle fiber. Methylene blue technique.

of the development and morphology of the motor end plate (1910) by the skillful use of a modification of the Bielchowsky technique, he found that after the nerve had penetrated the developing muscle syncytium, a true network of neurofibrils appeared. This was followed by a formation of the periterminal network in what was thought to be muscle tissue. This material did not stain black with silver but was seen as a yellow brown. Then a second motor plate much smaller than the first and also hypolemmal, formed itself in the vicinity of the first and was innervated by an accessory fiber which did not unite with the main medullated fiber. He considered the accessory fiber to be identical with the formation described earlier by Perroncito, Grabower, and Gemelli. In 1910 Botezat published a short note saying that his work on birds substantiated Boeke.

In 1913 Boeke reported that the accessory fiber in reptiles, birds and mammals ended in the neighborhood of the motor end plate. The accessory fiber differed from the main fiber in that it was not covered by Henle's sheath, contained no neurofibrils, did not enter the same plate with the myelinated fiber, but had its own characteristic ending. In *Sturnus vulgaris* I, this fiber was morphologically similar to the one which innervates the smooth muscle of the *M. ciliaris* of the human eye.

Three and four years later he (1916–1917) supplemented this suggestion by experimental degeneration. He cut the oculomotor nerve at its exit from the brain and killed the animal three to four days after. In the study of the ocular muscles the medullated nerve fibers were found to be undergoing degeneration, leaving their non-medullated accessory hypolemmal endings in great clearness (Plate 4, fig. 12). This beautiful experiment was ruined by subsequent inquiry. When three weeks were allowed between the resection and the sacrifice of the animal the greater number of the non-medullated fibers had undergone degeneration also. Boeke suggested that these were cranial autonomic and that they underwent degeneration more slowly than the medullated fibers. Those remaining he interpreted as sympathetic. Then he removed the superior cervical ganglion but this procedure did not cause the complete disappearance of the accessory plates, although he thought them to be fewer in number. There is of course some difficulty in accepting his interpretation that the non-medullated fibers which degenerated in the second experiment were Langley's cranial autonomic system, for these fibers are thought to leave the brain via the third nerve and to form synapses in the ciliary ganglia and should therefore appear intact after resection of the oculomotor nerve near its point of departure from the midbrain. They might rather be the *terminaisons en grappe* fibers described by Huber (1898) for eye muscles.

These inconclusive experimental results were cancelled by two papers published in 1919. Boeke and Dusser de Barrene extirpated four consecutive spinal ganglia (Th 6–9) and cut the corresponding posterior and anterior roots in the cat. Twenty-eight days later the autopsy corroborated their intention. Not a single medullated fiber was intact in the intercostal muscles examined. However, the non-medullated fibers and their end organs remained. Agduhr extirpated the *ganglion stellatum* in two cats and killed them four and six days later. In the six-day cat, the remains of degenerated non-medullated nerves could be seen in the interosseus muscles. Further, he performed the converse experiment and cut the posterior roots distal to the spinal

ganglion and the anterior roots in the *foramina intervertebralia*. The animals were killed five to ten days later. All myelinated nerves, both motor and sensory had undergone degeneration. Quite a number of intact non-medullated fibers were found, either in the bundles of degenerated spinal nerves or along the vessels. Such evidence would appear to clinch the original contention of Boeke that the accessory fiber was sympathetic in origin. But it might be well to remember that we do not know whether or not it is sensory or motor, although we do know that the cell body of the fiber under discussion does not lie in the dorsal root ganglion. The extirpation of the *ganglion stellatum* might destroy the sympathetic sensory afferent fiber as well as the motor efferent. At least we know that in two muscles studied the non-medullated fiber innervating skeletal muscle is not somatic.

In 1922 Boeke published a small paper on the innervation of striped muscle in reptiles (Ophidia). He described two endings, the typical motor end plate and a *terminaison en grappe*, which had a sole plate of its own. There was no direct relation of the neurofibrils with the contractile element , but there was a periterminal network whose delicate fibrils could be followed to the anisotropic band of the contractile fibers. He thought that some of these fibers were sympathetic.

The next year Kulchitsky published a paper on the nerve terminations in the python using the gold chloride and the methylene blue dip method. He said that he found the larger, clear fibers innervated by medullated nerves and the small granular fibers by epilemmal unmedullated nerves. Dart (1924) using the same preparations did not see this distinction so clearly. In fact, he figured (Plate 1, fig. 1) three instances of a *terminaison en grappe* and a medullated sole plate lying on the same fiber (taken from ventral mesodermal muscle). There is no way of reconciling these two statements. One observer has made a wrong observation.

However, it was Kulchitsky who startled Hunter's (1925) imagination to take a bold leap. On Latham's meagre finding (with the uncertain gold chloride technique) of a non-medullated collateral arising from a medullated fiber, Hunter dismissed the careful experimental work of Boeke and his collaborators. Latham (1925) himself said that he was unable to stain the nerve terminations to the muscle spindles, although they were present. His technique by his own statement was not adequate. Further, it will be noted that the accessory system with the exception of Bremer's plate was not reported by the earlier workers with the gold chloride and methylene blue technique. It is unfair to say that Boeke's and Agduhr's results cannot be confirmed until a comparison of the two techniques has been made by an experimenter who has mastered both.

Hunter said that Latham examined the muscles of the hind limb of goats, from which the lumbar sympathetic chain had been excised by Royle. "The preparations from the operated side showed in transverse section a number of small muscle fibers which were in marked contrast to those of average size in the same preparation, which were apparently normal." The difficulty is that in Latham's paper on the nerve endings from the goat, fowl and sea gull, he does not make it clear that he is comparing a normal with a sympathectomized muscle. The writer saw the preparations which Hunter brought to Chicago. One or two *terminaisons en grappe* could be easily identified and, as reported, appeared on a muscle fiber small in calibre. But when

Hunter was asked about the source of the preparation in question, no clear cut answer was forthcoming.

It is quite true, as set forth in the discussion of red and white muscle fibers, that the muscles of higher mammals are made up of a mixture of these two types, but it is not known whether or not the white is similar to the large clear fiber and the red, to the small granular one. To recall Hay's work, he found no essential difference in the innervation of a white and red muscle, except the size of the medullary sheaths. Were Hunter's generalization true the red muscles of the rabbit would have to be innervated by the sympathetic system.

It is an interesting observation upon the workings of the human mind to find Kulchitsky's article on the python continually quoted, and his work published later in the same year on the innervation of the muscles of the frog, entirely neglected. In his second paper he does not so much as mention the existence of a muscle spindle in that animal, but describes elaborately endings which he regards as sensory, although the older literature allows no mistake about the existence of spindles in the muscles of the frog. Further, he does not mention the well-known fact that the two types of muscle fibers are characteristic of the frog as well as of the python. Garven (1925) said that in the frog these two kinds of muscle fibers may be innervated by branches from the same axis cylinder (medullated). The writer found has the same with methylene blue technique (Plate 4, fig. 13) and has demonstrated that these fibers degenerate following the cutting of the nerve at its exit from the spinal canal. The sympathetic was left intact.

Garven studied in particular the *panniculus carnosus* of the hedgehog. He thought that in those muscle fibers there were varying degrees of longitudinal striation and varying degrees of content of sarcoplasm and that they could be divided into two definite classes. He reported that all the muscle fibers appeared to be innervated in the same way and Boeke's accessory fibers to be present. The latter reached the endings via capillaries. He found them in frog, lizard and human muscle. These are interesting morphological observations for they support Boeke's data with the gold chloride technique. Garven is undoubtedly correct in his estimate of the innervation of the *panniculus carnosus* in the hedgehog, but this particular case may not resemble the other cases cited.

In Q. Cushing Smith's (1926) comparative study of preparations of the facial muscles of the kitten and young guinea-pig with methylene blue technique, it was evident that the simple morphology of the muscle fibers themselves was greatly dissimilar. In the kitten's platysma there were single fibers resembling in morphology the red intrafusal fibers of other mammals, but not lying in connective tissue sheaths. They were innervated by a non-medullated nerve, while the other fibers lying about them had typical motor end plates originating from medullated axis cylinders. In the platysma of the guinea-pig, the morphology of all the fibers seemed to be the same. A rare ending similar to the *terminaisons en grappe* was found.

This is interesting in the light of a personal communication which Cobb (1925) quoted from Bielschowsky, who

> believes that the question of double innervation is still obscure in many ways and needs further study especially from the quantitative point of view. He finds that the end organs described by Boeke are most numerous in the external ocular muscles, less numerous in the facial muscles, and very occasional in the muscles of the extremities.

And yet it was the muscles of the extremities (Agduhr, 1918) and the intercostals (Boeke and Dusser de Barrene, 1919) which were used to prove their existence. To continue, Bielchowsky

> considers it proved that these are sympathetic endings (although certainly Boeke's work on ocular muscles is far from satisfactory in the writer's opinion), saying that the fibers may be recognized by the fact that they are non-medullated throughout their course, whereas the cerebro-spinal fibers only become non-medullated shortly before their junction with the end plate (p. 525).

In résumé, we find that Kulchitsky's work on the python is not supported by Boeke's on snakes, nor by that of Garven on the hedgehog, frog, lizard, and man, nor by that on the frog by Hines (1926). Hunter's separation of innervation of white and red muscle cannot be supported by the inadequate technique of his collaborator, Latham, nor by the work of Hay. In other words, Hunter's generalization does not appear to have an adequate foundation in fact. The whole field calls for reinvestigation with the prevalent techniques in the hands of one who has mastered them and also by one who thinks of nerve endings as organs of activity.

In the light of their uncertain appearance the anatomical facts have been exploited far beyond their factual meaning by the physiologist in search of an anatomical structure upon which to hang his theory of muscle tone.

THE FUNCTION OF THE SYMPATHETIC INNERVATION OF SKELETAL MUSCLE

From the previous discussion it is evident that the muscles selected for study have accessory endings from the sympathetic nervous system. Although there is a definite morphological and physiological difference between the red and white fibers of birds and mammals, and a morphological difference between the clear and granular fibers of lower vertebrates, there is at present no justification for the belief that Kulchitsky's finding of separate innervation of the two types of muscle in the python can be applied indiscriminately to the striated muscle of all vertebrates, nor that the red is equivalent to the granular and the white to the clear. It may be true, but the balance of the data has not settled into the static equilibrium of a proven fact.

Botazzi's (1897) theory that the sarcoplasm of skeletal muscle was responsible for the maintenance of a contracted state, superimposed upon it by the clonic contraction of the myofibrils, was expanded by Mosso (1904) to read that muscle tonus, a characteristic of the sarcoplasm, was maintained by the sympathetic. But what is tonus? Being an anatomist, the writer may not know in this time of specialization, when the right of definition lies only in the province of the specialist, and particularly when observation has found even students of tone to be completely uncertain. Suffice it to say that upon palpation a muscle feels different when all the nerves innervating it and the whole central nervous system of the animal are intact, than when any part of the nervous system related to it is injured. The earliest observation, lost in the obscurity of the past, but demonstrated as a fundamental physiological fact, that after section of its motor nerve the muscle not only loses its contractility but also its feeling of tautness when taken between the fingers remains true. Brondgeest (1860) made the earliest observation known to the writer that this tonus could be destroyed in a muscle by cutting the posterior roots, i.e., the sensory nerves in the region of its motor innervation. The fibers from the posterior roots run with the motor nerve and end

in the muscle spindle (Onanoff, and Sherrington). The maintenance of slight degrees of contraction necessary for the prolonged activity of the antigravity muscles has been thoroughly studied by Sherrington (1915). In that paper he said that reflex postural contraction was characterized by the low degree of tension it usually developed, its maintenance without obvious fatigue, the difficulty of obtaining a resemblance by artificial reflex contraction, the ease with which it is interrupted by reflex inhibition and the reactions of what he calls "lengthening" and "shortening." By the release of lower motor centers in the brain stem from higher control (Weed, 1914; Sherrington, 1898, 1915; Magnus and de Kleijn, 1924) it was found that the "tonicity" of the postural or antigravity muscles was increased beyond that of their antagonists, the flexors,—an experimental procedure, popular with all students of this subject. It is known as decerebrate rigidity. The climax of this differentiation between the two groups of muscles, flexors and extensors, in four footed laboratory animals appears when the phylogenetically old part of the midbrain *nucleus ruber*, cut off from higher centers, can no longer modify impulses from higher motor centers or to the motor cells of the spinal cord. This peculiar condition is modified by the incoming stimuli from the neck muscles and the semicircular canals of the internal ear, and by an intact cerebellum. It is destroyed by cutting the medulla in the region of the eighth cranial nerve (Magnus and de Kleijn, 1924) or by section of the posterior roots (Sherrington, 1897–98).

Thus, physiologically, a dual concept of muscle has arisen. It may contract in response to stimulus or it may maintain a contraction—the contractility and plasticity of skeletal muscle. Sherrington used for illustration of the latter character, the well known fact that the bladder is capable of relaxing and maintaining an almost similar intra-vesicular pressure whether it contains a hundred or two hundred cubic centimeters of fluid. Were the tissue dead, the stretching necessary for the accommodation of the increased amount, read in intra-vesicular pressure, would more closely approximate the increase of fluid. His analogy drawn from smooth muscle, innervated by the autonomic nervous system, together with the work of de Boer and Langelaan has stimulated the other hypothesis, that the sympathetic holds a peculiar power over the plastic quality of tone in skeletal muscle.

To distinguish between these two characteristics, the chemistry of muscle, its electrical response, its action current, its heat of contraction and the action of drugs upon it have been studied. At the present time no resolution of the data presented by workers in these various fields presents itself (see Coman, 1926, and Cobb, 1925).

De Boer (1913, 1914a and b) removed the abdominal sympathetic chain from frogs and cats and reported that the homolateral limbs hung much lower when the animals were held suspended. Even the tail of the cat was bent to the opposite side. Beritoff (1914) reported that there was no change in the tonic reflex activity of the posterior extremities of the frog when the *rami communicantes* of one sympathetic were cut. Dusser de Barrene (1917) found that when the abdominal sympathetic was removed a trifling but plain loss of tone in the muscles followed. It was not an atonia but a hypotonia which in no way resembled that produced by the section of the dorsal roots. It gradually grew less and finally vanished. Cobb's (1918) repetition of de Boer's experiments

offered no support. Van Rijnberk(1918a and b) found that there was no tone in the diaphragm on the side where the phrenic was cut as shown by x-ray nor could he obtain any contraction when the splanchnic was stimulated. Furthermore, he found that decerebrate rigidity always developed on the side where the sympathetic had been cut and that there was no difference in creatinine of decerebrate rigidity after the sympathetic was removed. From this type of experiment, on frogs and cats, it is impossible to conclude that the sympathetic as such has any thing to do with tonicity.

Langelaan's (1915) study of the elasticity and plasticity of muscle under such conditions as increase and decrease of load and its temperature variable bears little or no relation to other studies on tonus. Even his definition that tonus is equivalent to the increase in length of muscle per gram increase of load has no relation to the method of study pursued by other workers. In an atonic muscle this quality has two different elements, an elastic elongation and an after elongation, i.e. tonus is the sum of an elasticity and a plasticity. By means of many measurements and several formulae he concluded that elasticity is a constant and the variation of the tonus is due to plasticity. If we accept his assumptions and definitions and disregard the fact that the muscle is dying gradually, we must admit that his conclusion is the result of inexorable logic. Upon "stimulation of the pretibial muscles by a faradic current and the subsequent contraction of these muscles" there is caused "a considerable increase of tonicity of the *musculus gastrocnemius;* this effect may even persist some seconds after the cessation of the stimulus. The increment of tone is chiefly due to an increase in the plasticity of the muscle." (p. 295). This tonic contraction was not a single contraction but a tetanus. The curves which represent it resemble those of smooth muscle in their tetanic character, stair-case phenomena, slowness of contraction, and slight height. This he said is due to the sarcoplastic part of striped muscle which is innervated by a sympathetic motor cell. The conclusion is obvious, that the plasticity of muscle is dependent upon the sympathetic system. Thus the value of a painstaking study is lost in fantasy and the sympathetic bears away the control of that inexplicable attribute—plastic tone.

In 1922, Langelaan repeated de Boer's experiment and reported a partial but definite loss of tone, thus substantiating his previous observations. In the same year Ducceschi removed the superior cervical ganglion in the rabbit. He reported that there were inclination and deviation of the homolateral ear for many weeks, when the animal crouched at rest. If, however, it was excited both ears were contracted in the same degree. The ears of normal rabbits often may be seen in a position similar to those pictured by him.

Orbeli (1923) used the isolated and therefore bloodless caudal half of a frog to determine whether or not the response of the gastrocnemius muscle to electric stimuli of threshold value applied to the 8th and 9th anterior spinal roots was modified by another thrown into the 7th sympathetic ganglion at intervals. There was no change in the curve until the muscle became fatigued. If the muscle was tetanized for three periods each of five seconds, separated by fifty-five second intervals and then the sympathetic stimulated, the tetanus was held even at a higher level. He thought the sympathetic increased the force. amplitude and speed of development of contraction. He interpreted this effect as trophic. Thus does Pavlov's theory of trophic innerva-

tion gain a new life. Wastl (1925) obtained exactly contradictory results. In the anesthetized cat with intact circulation she stimulated the anterior roots of L 6 and 7 with break shocks at a rate varying from 100 to 220 a minute. At the periods of fatigue of the anterior tibial muscle the lumbar sympathetic was stimulated with faradic current. Six animals showed no change; in three the height of contraction decreased. Following an administration of a small amount of adrenaline a decrease and sometimes a slight increase in height of contraction resulted together with the anticipated brief rise in blood pressure. In the frog, she got no result either with stimulation of sympathetic or by the use of adrenaline. Probably the activity of the sympathetic cannot overcome the fatigue of striated muscle.

Hunter (1924, 1925) and his group of collaborators, with more enthusiasm than critique, have reviewed all aspects of the theory of sympathetic innervation of tone in striated muscle. It is impossible not to be impressed by Royle's results on human patients when the moving pictures of "before" and "after" are seen; just as impossible as it is to understand why the operations performed by the same skillful surgeon in Chicago and Boston did not yield the glowing results of the initial ones made in Sydney. There were three possible differences: first, the Australian patient may have been susceptible of suggestion; second, the skill of the surgeon may have increased; and third, the American patients were not submitted to as long periods of subsequent training. Kanavel, Pollack and Davis in Chicago, without the same type of selection removed the whole of the sympathetic chain in the lumbar or cervical regions and reported no subsequent change with one exception, that of a spastic hemiplegia. In that particular case, the pictures presented at the Chicago Medical Society (Nov., 1924) did show slight improvement, not greater, however, than Franz (1923) reports in his work on reëducation without operation. Carlson thought that the orthopedic surgeons were more optimistic about Royle's results than a physiologist would be, were he examining his own studies on laboratory animals. Crothers (1925) found the cases operated on by Royle in Boston disappointing. The results which followed Royle were not sufficiently encouraging to make this operative procedure advisable. So the hope of surgical intervention in cases of hypertonicity passed.

As for the goat (fig. 6, Hunter, 1925) no one has attempted a repetition of that striking experiment to see whether or not the sympathectomized limb refused to become rigid in the decerebrate animal, and what is more interesting, no one has used Royle's technique in removing the sympathetic influence from the limb of any other animal. It must be remembered that he did not remove the sympathetic chain, nor did he cut the grey *rami communicantes;* he evulsed them, that is he took hold of the ramus with a pair of forceps and pulled it out of the sheath away from its peripheral distribution. It is possible that with the increase in numbers of human operations he performed, the trauma became less, although the form of procedure remained the same. To the writer, this is the only explanation of the divergent results which followed the work of the same man. It may also account for the individuality of the goat. It is strange that they sought for no corroboration upon the common laboratory animals such as the cat or dog. Further, there is no record of the sudden onset of

fatigue in that animal, comparable with that which overtook the sea gull after a flight of an insignificant 100 metres.

In sea gulls and fowls Hunter (1925) performed the following experiment; in the first group he cut the sympathetic chain cephalad of the first thoracic segment; in the second, he sectioned the posterior roots of the first thoracic level; and in the third, he cut the posterior roots of the last four cervical levels. In the first, although considerable tone remained, the abduction and dropping of the elbow was apparent, and became more so under light anesthesia. The fixed position of the flexed wing could not be maintained nor could the bird fly attached to a light line on a smoothly running reel without marked fatigue. Where both wings were so treated, "after several short flights both wings were drooped and the bird was apparently exhausted, the mouth was widely open and the respiration very rapid." In another instance, "the wing was heavy and dependent as in flaccid paralysis." The result following section of the first thoracic posterior nerve upon the posture of the wing was the same as that of cutting the sympathetic chain.

The third operation removed "the tendency of the wing to take up a flexed position, that is, contractile tone is absent. If the wing is passively flexed it will remain in its new position. It has the properties of a plastic body, enabling it to remain in any position in which it may be placed." However, "where the wing is stretched outwards and released it drops to the side of the bird." In other words it was not completely a plastic body. It had lost the ability to oppose the force of gravity, which Sherrington thought so characteristic of posture. The normal mode of contraction, that is, extension of the wing in flight was lost. It was toneless in Sherrington's sense (Walshe, 1925). Walshe (see Adrian, 1926) examined one of the birds on which Hunter had operated. Not only did he think "that plastic tone" as defined by Hunter was absent, but "in addition, the wing was wholly flaccid in passive movement and showed no trace of tonic activity of any kind."

Now, if the first two operations give the same results, Hunter's hypothesis of plastic tone is dependent upon the intactness of the afferent sympathetic arcs. However, there is no proof other than this experiment for the distribution of the visceral afferent, and the distribution of the visceral afferent is used to explain the results. The reasoning is circular. Also, for the anatomical support of this explanation an afferent nerve fiber from the plastic or small muscle fiber is assumed (fig. 2, p. 11)—an assumption which cannot be rooted even in Kulchitsky's work on the python; but rests solely upon the questionable morphological pictures of Latham, that the slender muscle fibers are provided with both hypo- and epilemmal endings. No experimental data were offered. Hunter's theory requires two anatomical facts, not yet in existence, to prove that the afferent part of the reflex arc supports the maintenance of plastic tone. Further, were it true that the afferent and efferent sympathetic arcs connect with the cord via the first thoracic segment and maintain plastic tone, the crucial experiment to demonstrate it was not performed. The dorsal and ventral roots which innervate the skeletal muscle of the wing were not cut simultaneously. In other words, the sympathetic arc was never given complete autonomy. Its isolated function of innervation in birds was not studied.

This report for the hen and sea gull corresponds in no detail with that of Trendelenburg (1906) who divided four to ten posterior roots in the cervical and thoracic

region, thus affecting, according to Hunter's anatomy, both sympathetic and somatic afferents. The normal wing posture was retained and the flight was normal. There was no tonic resistance to passive stretching. When placed in an abnormal posture, it was not corrected. But if the normal wing was also placed in the same abnormal posture, both were returned to the normal. However, after bilateral sections, the resting position of both wings was normal, but superimposed abnormal postures were not corrected and flight became impossible. From this report, "the plastic body," the wing, results from a simple loss of incoming proprioceptive impulses; the motor cells can no longer react to stimuli from that side. It is not plastic when they are stimulated through the afferents of the heterolateral wing.

Kuntz and Kerper (1926) repeated the experiments of Hunter in fowls and pigeons with the same results. However, they severed the sympathetic trunks after section of the posterior roots. The wing no longer tended to remain in the position in which it was placed by the observer, but to hang dependent. These observers did not keep their birds alive 60 days—an experimental condition which is requisite for the defect in the wing posture to become gradually less apparent. It may be possible that the sympathetic exercises a control over posture in birds that is singularly at variance from that which will be described for mammals. Unlike the mammalian forelimb, the wing is covered with feathers and the innervation of their erectile muscles has been destroyed. That loss may mask to some extent a result which would otherwise appear as negligible as from the loss of the sympathetic in other animals. The most suggestive finding is that cutting the posterior root at Th_1 is equivalent to section of the sympathetic chain. It offers a means for determining something about the elusive peripheral afferent visceral ending. The movement of the feathers is one of the few motor responses resulting from stimulation of the cortex in the pigeon (Rogers, 1922). We note that neither observer studied birds under decerebrate rigidity.

Kuntz and Kerper found that under anesthesia a marked difference in the normal and sympathectomized limbs of dogs could be shown. If equal weights were attached to the two hind limbs, the normal was always less fully extended than the operated. Had they cut the dorsal and ventral roots innervating one hind leg, leaving the sympathetic intact, and then demonstrated that under ether, the operated leg and the normal supported an equal weight with the same degree of extension, they would have proven that the elimination of the somatic system by ether was equivalent to experimental resection. If this compared favorably with their previous experiments, the sympathetic would have been shown to subserve some discernible function in striated muscle. They used a modification of Spiegel's (1923) method for obtaining a tonus curve, that is a curve of passive extension of the quadriceps muscle, by what they call the break phenomenon, normally, that muscle offers greater resistance at the beginning of displacement of the limb from a position of rest than when the joint is slightly flexed. It was absent in the leg from which the sympathetic had been removed (from 2 to 112 days). Had they demonstrated by Spiegel's method that an extremity in which the sympathetic alone remained gave a curve similar to that of the intact limb under ether and therefore far removed from the straight line of the denervated limb, they would have approached more

nearly the proof of the sympathetic's responsibility for some part of the tonus of skeletal muscle.

Huggett and Mellanby (1925) found that an intravenous dose of curare, which did not paralyze the sympathetic system, produced complete flaccidity in the rigidity of decerebration, while Combs and Tulgan (1925), contrary to the emphasis placed by Hunter upon the interval between sympathectomy and the production of decerebrate rigidity, reported that immediately after removal of the stellate ganglion from cats, the rigidity of the forelimbs was very much less than that which was found to persist in the hind limbs.

Coman (1926) observed the effect on tonus of the forelimb in cats and dogs from (1) stimulation of sympathetic fibers passing to the limb, (2) section of the ventral roots of the spinal nerves of the thoracic sympathetic, (3) section of the ventral roots of the cerebrospinal nerves forming the brachial plexus. In the first group of experiments Coman said that

> In no animal was there any vasomotor, pilomotor or secretory response in the forelimb resulting from stimulation of roots higher than the fourth thoracic. Moreover, despite observation under the most favorable circumstances to rule out the vestibulocervical phenomena of Magnus and de Kleijn, in no case did stimulation of any of the thoraco-lumbar roots result in any detectable tonic effect in the limb musculature.

In the second procedure he found "no tonic, motor, or reflex dissimilarity in the forelegs, either immediately or throughout the largest interval before decerebration." There was no appreciable difference in the two sides in decerebrate rigidity and plastic tonus was not impaired. All the reflexes of posture were equally active. The animals from the third experimental procedure were kept not longer than fourteen days, in order to exclude the factor of muscle wasting.

> Following decerebration, the limb deprived of somatic efferents at no time showed the development of any postural reaction whatever, although normal decerebrate rigidity appeared invariable in the intact limb. Noxious peripheral stimuli, and stimulation of the cut brain stem were equally unproductive of any change in the denervated limb although, as before, the intact limbs reacted normally with augmentation of extensor rigidity. Similarly, passive adjustment of head and neck, while provocative of Magnus-de Kleijn phenomena in the intact foreleg, caused no reaction in the operated leg . . . . Passive flexion or hyper-extension of the operated leg resulted in one of the 'shortening' or 'lengthening' reactions of Sherrington. Plastic tone in the sense of Langelaan was not seen at any time in the de-efferented limb.

In these carefully controlled experiments, no proof is available of a relation between the sympathetic nervous system and either the development or maintenance of postural tonus in striated muscle.

Tower (1926) carried out a series of experiments on dogs in which the stellate ganglion was removed upon one side and the operation without removal performed on the other side. The normal activity of the animals in question was studied, some of them being kept ten months after sympathectomy. They were run (in a motor driven tread mill) for as many as fifteen miles, without great difference in fatigue of the two front limbs, "in fact the greater the fatigue the more alike was the behavior of the two limbs. The differences present at the end of the third mile were lost by the ninth and twelfth." They were studied not only in the decerebrate rigidity preparations of Sherrington and of Davis, but also under such conditions as the hypertonicity of parathyroid tetany and strychnine spasms. "After moderate muscular work, in strychnine poisoning, and in

parathyroid tetany, a tendency for the exhibition of greater tone in the sympathectomized limb was found." There was sufficient evidence of "a quantitative relationship between blood flow and tone to be very suggestive that the circulation through the limb is one of the factors conditioning the tone of the limb." Certainly, the test for fatigue was much more severe in these experiments than in those reported by Hunter on birds.

The evidence presented shifts the balance of responsibility for tone away from the sympathetic. Its removal does not affect the development of decerebrate rigidity, nor modify the reflexes governing posture, nor in the most strenuous tests in mammals does it shorten the time of development of fatigue. At present, in spite of Orbeli's deduction there is no proof of its trophic influence. But there it is, innervating striated muscle. Further, its participation in the muscle spindles of mammals, although suggested by Agduhr, has not been proven except in the innervation of the capsule. Of the part it plays on the intrafusal fibers of birds, nothing is known. There has been no experimental work that proves Kulchitsky's exclusive sympathetic motor innervation of the spindle of reptiles. No other worker has reported a similar finding in that group of vertebrates. The proof of difference between the innervation of red and white muscle fibers in birds and mammals awaits more adequate data for the confirmation of Hunter and Latham. Such a variation in innervation either escaped Boeke and Agduhr, careful observers that they were, or they were unaware of the necessity of making the observation. Of course, this does not prove its nonexistence. The sympathetic, then, is found in some skeletal muscles at least, without anything assigned for it to do. Just how much it participates in the innervation of striated muscles, their contained spindles or other sensory endings, just how that innervation varies from one type of muscle to another and from one animal phylum to another is not definitely known. We are at a greater loss than Mosso and Botazzi to explain its appearance at all, for their supposition, although it has stimulated more observations and theories than the writer has reported, remains one of those theories which has released the biologist's imagination and in the end fails of adequate proof. The same may be said of the Hunter and Royle generalization, magnificent in conception, impossible of proof, and yet the work which it initiated, although it cries aloud for the most part "untrue," is its inevitable result. Perhaps, this review will point out the inconsistencies in our knowledge concerning nerves and muscle in such a manner that work will not be slow in forthcoming to reconcile them. Nevertheless, we must thank the four workers just mentioned for the stimulus of their mistakes, born of insufficient thought and inadequate data, but making an appeal to the imagination which is of major importance. If the reader wishes a poetic definition of tonus, let him read that of Cobb (1925) in the last paragraph of his review. It is full of rhythm and cadence. Also it holds all that we know how to express at this time of that subtle and characteristic quality of normal muscle, tonus. The writer has found no scientifically limiting definition, nothing for instance which matches the physicist's circumscribed concept of force or mass. And until then we see that which is true of tonus through a glass darkly.

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ANIMAL LIFE IN HOT SPRINGS

By CHARLES T. BRUES

Harvard University, Bussey Institution for Research in Applied Biology

SO CLOSELY does the tangled web of life spread over land and sea that it is well-nigh impossible to pick a spot not peopled by animals and plants. Nevertheless, the distribution of any species is determined by the interaction of a series of factors. For the purposes of analysis, we may regard these more or less naïvely as independent and capable of exact statement. Most positive in their action are the limitations set by such physical barriers as oceans and mountain ranges in determining boundaries beyond which most animals and plants cannot pass except under exceptional circumstances. Moreover, such barriers have generally persisted over long periods of time and within the areas thus circumscribed there have arisen characteristic faunas and floras that are quite distinct one from another. On account of a gradual increase in the extent and efficacy of some of these barriers during geological time and an almost uninterrupted action of others over long periods, it follows in a broad way that species are most clearly restricted, genera less so, families still less, and that larger groups tend to be more cosmopolitan. Such a very general statement is by no means accurate in detail since the age of the several groups, their ability to migrate and many other circumstances tend to upset such a simple arrangement. The action of ocean barriers is clearly seen in the faunas of oceanic islands or of large isolated land masses like Australia and Madagascar. Mountain ranges form much less efficient means of herding animals and plants together and their action is in part dependent upon the fact that they interpose a strip of higher, colder land between two lower areas. On the other hand they frequently produce great differences in rainfall between the lowlands which they transect.

CLIMATIC FACTORS IN DISTRIBUTION

The importance of climatic differences in limiting the distribution of animals and plants is dependent mainly upon two factors which are to some extent independent, upon temperature and humidity, the latter in turn determined roughly by the rainfall. Thus the distinctness of the fauna and flora of temperate North America and tropical Central America is in great part a function of temperature, while that between the middle Atlantic States of the same continent (moist transition zone) and the Great Plains (arid transition zone) is due to quite consistent differences in humidity. In neither case do physiographic barriers play a part.

Extremely low temperatures during a considerable part of the year are no deterrent to the successful existence of either plants or animals of many kinds so long as there is a sufficiently long warm season for them to grow and reproduce. Likewise an almost total lack of rainfall and a persistently low relative humidity are equally congenial to the varied assortment of plants and animals that are

adapted to life in deserts, of which there are numerous examples scattered over the globe, each with its quite distinctive fauna and flora.

Compared with their adaptability to intermittently very low temperatures or to continuous dryness, the upper temperature limits compatible with existence, particularly that of animals, are much more rigidly fixed. The maximum warm weather temperatures of cooler climates do not differ greatly from those of the tropics, however greatly the winter minima may drop below the continuous warmth of a tropical climate. Aside from ocean temperatures in the colder seas or currents and in extremely cold climates, the maximum temperatures of the water inhabited by the vast array of animals and plants of aquatic habits vary in comparatively low degree.

FIG. 1. LONE STAR GEYSER, YELLOWSTONE PARK

The cone is of siliceous sinter that has been built up of silica deposited upon growing algæ.

There are, however, notable exceptions to the rather uniform temperature conditions which prevail during the season of growth and reproduction in the ordinary aquatic habitat. These are presented by thermal waters or hot springs, the fauna and flora of which shed considerable light upon the temperature requirements of living organisms and their ability to adapt themselves to the higher temperatures and other unusual conditions which prevail in these springs.

OCCURRENCE OF THERMAL SPRINGS

Thermal waters or hot springs occur in many parts of the world, most frequently in places which show signs of volcanic activity. Numerous groups are scattered over Europe; there are famous ones in Iceland, New Zealand and Algeria, and in many countries there are others of minor extent or more poorly known, while there is no doubt that many remain to be discovered or at least accurately described. The temperature of these springs varies greatly and their rate of flow is extremely varied. Some issue violently at intervals from the earth as geysers, emitting superheated water which gives off much steam. Others contain boiling water, but the majority are of lower temperature and these grade down to springs which are hot, warm, tepid, or only to be recognized as of thermal nature by means of a thermometer.

In the United States there are a number of areas with thermal springs. In New York and Virginia there exist a few of very moderate temperature. In Arkansas there are others, but the most extensive

and those exhibiting really high temperatures are in parts of California, Colorado, New Mexico, Utah, Nevada, and finally in Yellowstone Park, Wyoming, which contains the largest and most varied group of hot springs in the world. Practically a continuous series with respect to temperature is to be found in Yellowstone Park. From the intermittent geysers (fig. 1) water at boiling temperature is poured forth, and certain boiling springs or pools are equally hot in places where the water is in active ebullition. The outer margins of these pools are cooler and the overflow waters gradually cool as they run off in small streams or spread over the surface of the ground, finally to reach as low a temperature as that of streams or pools of direct meteoric origin. Other pools or ponds contain water of more uniform temperature far below the boiling point (fig. 2) and often scarcely warmer than ponds of non-thermal origin.

FIG. 2. BATH LAKE, YELLOWSTONE PARK

A warm pool fed by a hot spring near one edge. The temperature of the water ranges from about 90° Fahr. to much higher where the spring enters at the far side.

BIOLOGICAL PECULIARITIES OF HOT SPRINGS

The biological characteristics of thermal waters depend upon several factors. Of these the most obvious is the high temperature of the water. The temperature of the medium is a most important consideration and this is the factor which has most generally attracted the attention of biologists. Before the beginning of the nineteenth century Saussure (1796) noticed in certain European hot springs that there were both plants and animals living in water at temperatures well above those which obtain in places ordinarily populated by living organisms, and since that time it has become more and more clearly

apparent that the inhabitants of hot springs are in some way endowed with powers of resistance to heat that is fatal to the ordinary run of animals and plants. The exact nature of the physiological adjustments of this thermal fauna and flora has so far not been satisfactorily determined, although several theories have been advanced to account for the adaptations which undoubtedly exist.

Another common characteristic of thermal waters, particularly of those which emerge from the earth at very high temperatures, is the small amount of dissolved oxygen. As a result of this condition the fauna is further handicapped by the dearth of this essential element, except in the case of certain animals, e.g., the adults of aquatic beetles or the larvæ of certain Diptera which secure their oxygen not directly from the water, but from the atmosphere above its surface.

Thermal water is also commonly impregnated with salts in solution, and the nature of these is naturally very dissimilar in different places, since it is dependent upon the constitution of the rocks with which the heated water has been in contact. In the Yellowstone Park springs there are two predominant types, one containing calcium carbonate (fig. 3) and the other charged with silica (fig. 4). In the former case the water is acid in reaction, due to the presence of much carbon dioxide, and in the latter the silica is dissolved in the presence of sodium carbonate. Other salts that are frequently present are sodium sulphate, sodium chloride and gypsum, with occasionally appreciable amounts of arsenical materials. The

FIG. 3. A PORTION OF THE MAMMOTH TERRACES, YELLOWSTONE PARK

These are formed by deposition from water containing dissolved calcium carbonate. Algæ occur in these waters at 162° Fahr. and below, imparting much color to the deposits.

addition of such substances to the water is naturally an important environmental factor and produces conditions similar to those existing in alkaline lakes or ponds.

Hydrogen sulphide is frequently present in considerable quantity, and sulphur dioxide is emitted from certain steam vents. After solution in water and oxidation sulphuric acid is produced and causes the charring of wood near such openings.

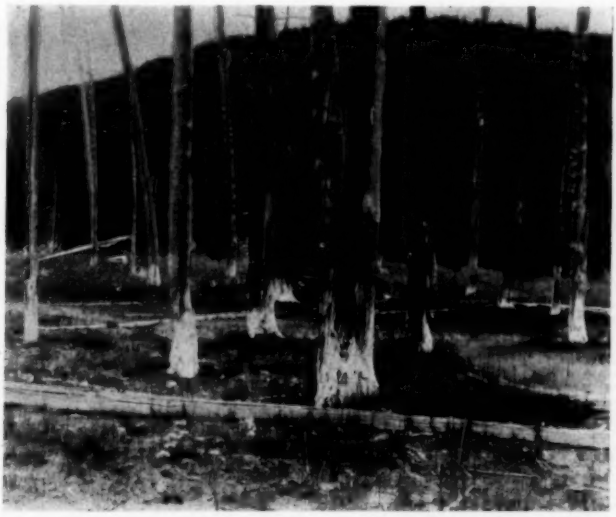

FIG. 4. STANDING TREES IN YELLOWSTONE PARK KILLED BY THE OVERFLOW FROM A HOT SPRING CHARGED WITH SILICA

Fallen logs and the stumps of standing trees are silicified by deposition from the water that rises by capillary attraction. Many deposits having a similar origin are known from the Tertiary of the Western United States.

Occasionally there are also noticeable amounts of finely divided mineral matter present in suspension in pools of tepid water, while in those which are in actual ebullition without much flow of water this liquid may become thick and pasty resulting in mud-geysers or "paint-pots."

The ecological peculiarities of the Yellowstone thermal waters are further enhanced by the cold climate of the park, which is located near the 45th parallel of latitude. The hot springs are at an altitude of between 6,000 and 7,000 feet, which thus places them within the Hudsonian life-zone. In the thermal waters and also in the soil immediately surrounding them much higher temperatures of course prevail throughout the year than are normal for this zone and the rigors of the winter are so mitigated that certain animals and plants have found an opportunity greatly to extend their northern range, irrespective of those that seem to be specially adapted to life in or about the hot springs.

THE BIOTA OF HOT SPRINGS

As has long been known, certain plants occur in water of much higher temperature than that at which any forms of animal life are able to exist (fig. 5). Several botanists (Davis, '97; Miss Tilden, '98;

Setchell, '03) have made observations of the temperatures at which living plants occur in the thermal waters of Yellowstone Park. Of these Setchell's account is the most complete and embodies a long series of observations on springs of different types. He found in the calcareous springs that chlorophyll bearing algæ occur regularly in water at 60°–63°C. (140°–146° Fahr.) and that forms destitute of chlorophyll extend into still hotter water of 70°–71°C. (158°–160° Fahr.). These coincide almost exactly with temperatures of 145° and 160° observed by us during the summer of 1923 and with a maximum of 160° reported years ago by Wood ('68) in Owen's Valley, California. In silica-charged waters still higher temperatures are compatible with plant life, as green plants grow in water of 75°–77°C. (167°–171° Fahr.) while those without chlorophyll occur in water as hot as 89°C. (194° Fahr.). At high altitudes like that of Yellowstone Park, this is practically the boiling temperature of water.

In the thermal springs of Europe many observations have been made, dating back to those of Saussure, who noted in 1796 the occurrence of plants in the waters at Aix at temperatures of from 35°–46°. As early as 1854 De Laures ('54–'55) determined that higher algæ ("conferves") grew in the thermal waters of Néris at temperatures of from 42°–48°C. (107°–119° Fahr.) and he further made the interesting observation that the growth of these plants was augmented during the summer, which he interpreted as due to the influence of light over longer daily periods. In Italy, Hoppe-Seyler ('75) found at Montegrotto near Abano in northeastern Italy algæ growing in water at 50°C. (122° Fahr.) and de Varigny ('93) refers to observations by Martens of *Oscillatoria*

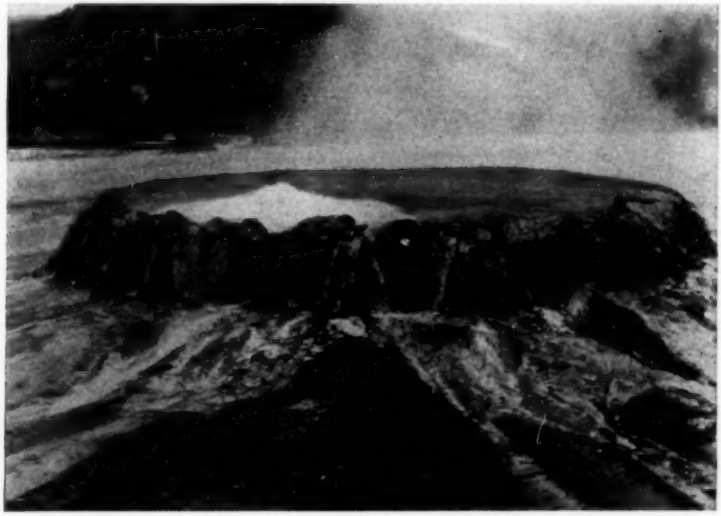

FIG. 5. "PUNCH-BOWL," YELLOWSTONE PARK

A boiling spring which has built up a circular siliceous crater. The overflow water supports growths of algæ which cause a streaking of the sinter deposit with reddish, orange and green.

in a hot spring on the Island of Lipari at a temperature of 55°C. (131° Fahr.). A slightly higher temperature of 60°C. (140° Fahr.) is reported for *Oscillatoria* by Schnetzler ('89) at Carlsbad. In the condensed water of steam caves in Ischia Hoppe-Seyler (*l.c.*) found green algæ at a temperature of 64.7°C. (149° Fahr.). In Iceland, several botanists have given accounts of the algæ which occur in the hot springs associated with the well known Iceland geyser region. West ('02) reports filamentous algæ in water at 85°C. (185° Fahr.). These observations accord closely with those made in America.

Concerning hot springs in more distant parts of the world there are few records which appear to be fully trustworthy. In the Himalayas, the eminent botanist Hooker ('54) reported "*Leptothrix*" in the waters of Thibetan hot springs at a temperature as high as 75.5°C. (168° Fahr.). Algæ have been found at Pugha in Thibet at 78.8°C. (173° Fahr.) and Dana noted algæ obtained at 160° Fahr. (72°C.) on the island of Luzon. In South America he speaks of vegetation in hot springs at Las Trincheras in Venezuela occurring in water as hot as 71°C. (160° Fahr.).

In the extensive hot springs at Hammam-Meskoutine in Algeria, Gervais ('49) found algæ in the second tier of basins of the cascade where the water registered 57°C. (135° Fahr.), but the upper series with a temperature of 63°C. (146° Fahr.) was without them.

Some of these records refer to green algæ, and others like the one at Las Trincheras undoubtedly relate to species without chlorophyll. With this in mind, it is evident that they agree closely with what is known of the algæ and other primitive plants in the thermal waters of Yellowstone Park, with the possible exception that no European species without chlorophyll appear to have been reported from such extremely high temperatures as in other parts of the world.

TEMPERATURE REQUIREMENTS OF ANIMALS

The temperature requirements for animals of all kinds are much more exacting than those for the lower plants and will be considered more in detail in a moment. Compared with plants their powers of adjustment to high temperatures are considerably less, and even the most resistant forms of animal life are unable to endure the degree of heat at which certain plants thrive.

Practically all groups of animals that are represented abundantly in fresh water contain a few species known to inhabit thermal waters. These forms thus make up an extremely varied although not by any means a rich fauna, similar in some respects to that of a volcanic island that has been peopled by visitors from the outside world.

From data furnished by experiments on certain marine animals it is evident that those forms which have always lived in the sea where they are never called upon to combat great fluctuations in the temperature of the water are very susceptible to injury from excessive heat. Experiments upon a variety of marine animals like crinoids, actinians, ctenophores, worms, crustaceans, molluscs and fish with respect to the temperatures they can withstand without immediate harm when the medium is slowly heated show that temperatures of 25°–30°C. (77°–86° Fahr.) are the highest that can be borne even for a few hours, although the mollusc, *Murex* (32°), and some actinians (38°) exceed these figures. These critical points apply quite closely to such other marine metazoa as have been examined.

As there are no marine forms occurring in hot springs it is of course impossible to

say what their powers of temperature adjustment might be after acclimatization over long periods, but when transferred from a uniformly cool environment it is evident that they are quite susceptible to heat and that forms like the actinian and snail just mentioned, which must normally be subjected to excess heat in the littoral of the sea, show the greatest resistance. The larvæ of certain marine littoral animals withstand higher temperatures. Some molluscs, annelids, trematodes and crustaceans are frequently killed by exposure at 30°–31°C. (86°–88° Fahr.), while others withstand 37°C. (97° Fahr.). Embryos of the common pond-snail, *Lymnæa*, survive a heating of the medium to 44°C. (110° Fahr.).

On account of the great fluctuations of temperature in all except the largest bodies of fresh water, the animals which inhabit them are periodically subjected to considerably higher temperatures than those encountered by marine forms, and we find that the fresh-water fauna is much better able to withstand extremes of heat. Observations on certain Amphibians by Davenport and Castle ('96) and others show that these animals are unharmed by temporary heating of the medium to about 38°C. (102° Fahr.). In thermal springs the tadpoles of certain tailless batrachians have been found living at temperatures noticeably above this and as these are species known normally to inhabit waters of meteoric origin, it is evident that they have become acclimatized to these higher temperatures by exposure extending over a number of generations. Fishes are more susceptible to heat, although there is evidence that certain forms have been acclimatized in a similar way. Even fresh-water turtles occur in hot springs according to Schmarda ('53), who cites the finding of these animals in Tunis in springs near the ancient Utica at 44°C. (111° Fahr.) and near Lenkoran on the Caspian Sea in sulphurous springs at 40°C. (104° Fahr.). Among invertebrates the molluscs and insects are most abundantly represented in thermal waters and, as indicated in detail later, certain members of these groups extend into water of 40°C. (104° Fahr.) and several degrees higher in a few instances.

ACCLIMATIZATION TO HIGH TEMPERATURES

The range of temperature over which the process of acclimatization may extend is nevertheless very limited and moreover very uniform among quite diverse animals. This is beautifully illustrated by the classic observations of Plateau ('72), who determined experimentally the thermal death point of a number of fresh-water arthropods known to occur in thermal waters as well as in ordinary ponds and streams. When we compare the temperatures at which these animals actually occur in hot springs with the highest temperature that that can be successfully endured by unacclimatized individuals of the same species we find that the possible range of acclimatization although not great is of quite consistent magnitude. This is evident from table 1 compiled from tables given by Plateau.

Practically all of the species listed above show that there has been an adjustment which enables them to live continuously at temperatures higher than those at which they are able to remain alive for more than a few minutes when taken directly from fresh water at ordinary temperature. The differences amount, however, to only a few degrees in each case and we may conclude that the range of temperature adjustment is ordinarily only from one to several degrees centigrade.

The Protozoa are the most resistant of all animals, and rhizopods and flagellates have been found in Italian hot springs at

54.5°C. and 51°C. (130° and 124° Fahr.) respectively by Issel, who also discovered an amœba, apparently the common *Amœba limax*, in the water at 50°–52°C. (122°–126° Fahr.) and ciliate infusorians at 46°C. (115° Fahr.) ('00, '01, '10).

THE FAUNA OF THE DESERT

The exact relations of terrestrial and aerial animals toward the temperature of the soil and air about them are much more difficult to determine than is the case with aquatic forms. Several reasons for this are self-evident. The specific heat of water is much greater than that of air and soil and the closeness of contact with the soil is so much less than in the case of water that the body heat of cold-blooded animals responds far more quickly and accords very closely with that of the water. The cooling effect of evaporation can also greatly reduce the body heat of non-aquatic animals. Finally, the vast differences between sunlight and shade temperature are in turn modified by the external color of the animals themselves. With such a mass of variable factors, coupled with the fact that most animals may seek the sun or shade at pleasure and may choose between a period of diurnal or one of nocturnal activity, the actual temperatures to which such animals are subjected and the periods of time involved are to be determined only approximately.

In nature the most obvious field for comparison with the aquatic thermal environment is the desert. Desert animals are at times subjected to high temperatures which are quite comparable to those encountered by the inhabitants of hot

TABLE 1

Range of temperature acclimatization

SPECIES	HIGHEST TEMPERATURE AT WHICH UNACCLIMATIZED INDIVIDUALS REMAIN ALIVE	TEMPERATURE AT WHICH INDIVIDUALS HAVE BEEN FOUND IN THERMAL WATERS	LOCATION OF THERMAL SPRINGS
Insects:			
Mosquito larva—*Culex pipiens*	38°C.	40°C.; 39°C.	Néris; Luxeuil
Mayfly nymph—*Cloë diptera* (nymph)	44°(?)	45°	Luxeuil
Water-bugs:			
Notonecta glauca	37.5°	45°	Vichy
Nepa cinerea	43°	45°	Vichy
Water-beetles:			
Agabus bipustulatus	36°	38°	Ems
Hydroporus dorsalis	39°	42°	Loèche-les-Bains
Hydaticus transversalis	38°	39°; 39.2°	Luxeuil; Ems
Hydrous caraboides	38°	40°	Néris
Arachnids:			
Argyroneta aquatica	37°	38°	Ems
Hydrachna cruenta	44.5°	46°	Luxeuil
Crustaceans:			
Asellus aquaticus	38°	42.5°	Loèche-les-Bains
Daphnia sima	32.5°	25°	Luxeuil
Cypris fuscata	35°	34.4°; 36°	Piedmont; Gastein
Cyclops quadricornis	35°	34.4°; 36°	Piedmont; Gastein

springs. Here the most important difference lies in the opportunities enjoyed by the members of the desert fauna to escape the excessive heat (and also the dryness) of the surface of the soil by burrowing beneath it during the warmer parts of the day. According to Buxton ('23) the surface temperature of the soil of the desert may normally reach 122°–140° Fahr. (50°–60°C.) in parts of Palestine, while even of higher readings of 78°–84°C. (173°–184° Fahr.) have been obtained in other desert regions. He also noted the occurrence of certain insects, nymphs of two species of *Mantis*, and adult grasshoppers, actively moving about on the ground where the superficial temperature was 50.8°C. (121° Fahr.) As the opportunities for bodily temperature reduction are very slight in such chitin-encased animals devoid of any special regulatory apparatus for this purpose and possessing only a very scanty supply of water, the conclusion appears inevitable that the temperature of their bodies cannot regularly be far below that of the soil surface. Buxton ('24) has actually taken the bodily temperatures of certain desert insects by means of a minute thermocouple which can be inserted *per rectum* into the body of the insect. By this ingenious device he has been able to determine that the body temperature may approach closely to that of the heated soil, but that it always appears to remain slightly lower. Thus in the black tenebrionid beetle, *Ademsia ulcerosa*, body temperatures of from 36°–39.5°C. were found where the substratum registered 38°–44°C. and in several pale colored insects the body was somewhat cooler in proportion, registering 33.5°–39.5°C. when the surface of the soil was 39.5°–45°C.

It is evident from these and other observations that the temperatures tolerated by desert insects correspond closely to those endured by the inhabitants of hot springs.

THE THERMAL DEATH POINT

Under experimental conditions the effect of certain modifying factors may be eliminated and this has been done to some extent by several entomologists who have determined the reactions to excess heat and thermal death-point of various unacclimatized insects. Cantoni ('72), Bachmetjew ('01), Dewitz ('06) and others have found that the larvæ of various moths and flies will tolerate temperatures of 40°–45°C. for more or less prolonged periods or somewhat higher ones (47°C.) for shorter ones. Most of these experiments place the upper limits of temperature tolerance in the neighborhood of 40°–41°C., or 104°–105° Fahr.

In connection with his studies upon the symbiotic intestinal protozoan fauna of termites, Cleveland ('23; '24) has examined with great care the reactions of both termites and protozoa to high temperatures. He found that the thermal death point of the intestinal protozoa is 36°C. (97° Fahr.) while that of practically all of the species of termites with which he worked was considerably higher. The host termites were entirely unaffected by a 24 hour exposure to 36°C. and were able to withstand without injury exposures of ten minutes to 42°–47°C. (108°–117° Fahr.). He thus regards 48°C. as the thermal death point of the termites, although with longer exposures this would no doubt be lowered to some extent.

The very general agreement of nearly all of the observations relating to the fauna of thermal springs with experimental data secured from other animals of the most diverse sorts shows that the tolerance of all animals to high temperatures coincides within quite narrow limits. These

would appear to range between 40° and 45°C., or 104°–113° Fahr., usually nearer the lower figure, except in the case of marine species, to which as already stated temperatures above 25°–30°C. quickly lead to death. There is also a close agreement between the limits above indicated for non-marine animals and the body temperatures of warm-blooded animals. As the latter are equipped with a thermoregulatory mechanism and are of course not subject to the fluctuations of the medium in which they live we must look for abnormal conditions to determine the upper limits of body-temperature compatible with continued life. Such conditions are furnished by individuals, including those of our own species suffering from acute febrile diseases, where the failure of the regulatory apparatus allows the body temperature to rise. Under such conditions a rise of body temperature of a few degrees above the normal level of 37°–42°C. (99°–108° Fahr.) is fatal to the animal. This is in close agreement with the reactions of the lower animals toward heat, especially when we consider that the homoiothermal animal must be regarded as acclimatized to high temperature; or, in other words, that the warm-blooded vertebrate normally maintains a body temperature very close to the thermal death point.

This uniformity of reaction among the most diverse animals would seem to be due to the fact that high temperatures act directly upon their protoplasm, or at least upon cell metabolism, and this view finds support in the experiments of Vernon ('99) who found in a series of both vertebrates and invertebrates that the voluntary muscles pass into a condition of rigor at temperatures approximating the lethal temperature limit. Unfortunately no satisfactory understanding seems to have been reached concerning the physiology of this process, for upon it must depend any theories relating to the acclimatization of animals to high temperatures. Davenport and Castle ('96) advanced the theory that the increased resistance of organisms acclimatized to heat is due to a partial dehydration of the protoplasm whereby on a purely physical basis, its coagulation point is raised in the temperature scale. This explanation is based partly on the fact that albumen coagulates at higher temperatures as its water content is decreased, and in part on the resistance of the spores of many microörganisms to temperatures far above those that can be withstood by their actively growing stages. That the same condition holds true in encysted or desiccated infusorians and even in minute metazoa like rotifers and tardigrades is also apparently well authenticated. In these animals the amount of exsiccation without loss of vitality is surprisingly large. Even in frogs it may proceed to a point where as much as 41 per cent of the body weight may be lost (Hall, '22) and subsequently regained without ill effects; such an occurrence normally follows the periodic changes of the season in certain Australian desert frogs. Setchell ('03) from his study of the thermal flora is not inclined to accept this view of exsiccation, at least in regard to the thermal algæ. Applied to the thermal fauna this theory would imply a decreased water content in the blood and tissues and it seems probable that their saline content in most invertebrates is actually greater, due to the excess of salts in the medium which pass into the blood and tissues (Frédéricq, '04). That an actual change of constitution such as coagulation in a substance like albumen through heat can apply directly to account for acclimatization in animals is very improbable for the lethal tem-

peratures for animals are very much lower than those for plants.

ORIGIN AND RELATIONSHIPS OF THE THERMAL FAUNA

It is evident that those fresh-water types of animals which are able to adjust themselves to the highly saline water of brackish ponds or of the ocean itself are those which most generally occur in thermal waters. The similarity between these two faunæ and that of the littoral zone of the sea also is very striking and cannot fail to impress any zoologist who cares to examine the thermal fauna in any detail. It may be said that a great majority of the animals which occur in thermal waters have close relatives which live in alkaline, saline or brackish water or even in the sea. As the only similarity between all such situations lies in the considerable amounts of soluble salts in the water, irrespective of their nature, it seems reasonably evident that the fresh-water groups of animals that have developed species adapted to thermal waters have done so through their ability to adjust their metabolism to the increased osmotic pressure of the medium. That the members of a number of small, quite unrelated groups possess this ability to an unusual extent is equally evident. That much variation occurs among animals in this respect is of course well known (*cf.* e.g., Garrey, '05; Sumner, '06), and the cases cited indicate that it is not safe to draw broad conclusions regarding extensive groups of animals from experiments with members of isolated genera or families.

In practically all thermal waters there is undoubtedly a considerable deficiency in dissolved air and a dearth of oxygen compared with meteoric waters, as gases are naturally in great part expelled from the highly heated waters when not under pressure. Thus in the overflow from geysers and in pools rapidly supplied by water from boiling springs the process of cooling to temperatures compatible with animal life takes place quickly and there is little opportunity for atmospheric air to be absorbed. The green thermal algæ growing in such water are a source of oxygen supply but the amounts which they are able to furnish to rapidly moving water must be slight even where they are present in a considerable quantity. In the hottest water non-chlorophyll bearing plants (like the sulphur bacteria, *Beggiatoa*) of course furnish no oxygen and their occurrence indicates the presence of some oxygen in the water when it reaches them.

As already mentioned, many of the animals, e.g., many insects and snails, are not entirely dependent upon the dissolved oxygen as they obtain atmospheric air. Nevertheless some are entirely dependent upon what they extract directly from the water, like the crustacean, *Gammarus*, and the larvæ of the fly, *Chironomus*, and those of water-beetles. In reference to the fauna of fresh-water lakes it has been conclusively shown that the amount of dissolved oxygen, which varies at different depths, is an important factor in determining the distribution of fishes and certain water-breathing invertebrates (Juday, '09; Juday and Wagner, '09). It is interesting to note that at least one type which Juday found to be characteristic of the lower water layers of Wisconsin lakes occurs in what appears to be one of the most anaërobic situations where I have found living animals in the thermal waters of Yellowstone Park. These are the larvæ of *Chironomus*, commonly known as blood-worms on account of the red hæmoglobin pigment which they contain.

The thermal fauna has, as already stated, undoubtedly been derived mainly

from an assemblage of fresh water animals that have found a suitable if not ideal environment in more or less saline waters of abnormally high and uniform temperature. We cannot, however, consider it at all homogeneous either in origin or in constitution. On the basis of our conception of the geological development of the earth we have only to go far enough back in the evolution of life to reach the point when all animals and plants belonged to a "thermal fauna." Just how elaborate and highly differentiated living organisms may have been at the time when all water on the earth was at a considerably higher temperature than at present is by no means to be stated. Our knowledge of fossil animals does not, however, suggest in any way that highly evolved types were present at that time and certainly no one would entertain for a moment the idea that arthropods or molluscs were then in existence. Even if other evidence did not preclude such a possibility it would therefore be fallacious to trace any great part of the present thermal fauna to a primordial one. Schnetzler ('89) would trace some of the highly adapted lower thermal plants like *Oscillaria* and certain diatoms to a primordial flora, an assumption which is far more plausible.

There is no doubt, however, that thermal springs like those now extant have existed in the past, as there remain widespread evidences of their activity. It is well known that many fossil plants owe their preservation to the deposition within their tissues of silica derived from hot waters charged with silica in which they have been immersed or from which they may have imbibed the solution by capillary attraction. The "fossil forests" of tree stumps, logs and bits of wood have originated in this way, and in one instance at least it is evident that deposits of fossil insects are to be attributed to the same source (Cockerell, '15). In the Gurnet Bay deposit on the Isle of Wight there is an accumulation of this sort which includes many insects in addition to numerous molluscs. These were perhaps caught in a mud spring, with heated waters, into which the insects fell, overcome by gaseous emanations.

This Gurnet Bay formation is early Tertiary, probably lower Oligocene and in connection with the present day fauna of thermal springs it is interesting to compare the forms which have been found there. Thus in his account of the insects, Cockerell finds two species of *Ephydra* and *Stratiomyia*, both of them genera known to contain modern species that breed in thermal as well as saline waters. In addition there are winged ants and other Hymenoptera, quite similar to the miscellaneous assortment of insects that fall into hot springs. Certain deposits of silicified wood are much older and the preservation of some Cretaceous and even Jurassic wood is so similar to that now in process of silicification that there can be little doubt that the hot springs which furnished the silica were so like those of the present day that they offered similar opportunities for animal life.

On the basis of these facts it may be stated that very probably there have always been hot springs and that they have presented quite similar conditions for a long time, but that their fauna has had its origin in animals from cool fresh water that have become adapted to life in the higher temperatures and more saline water of thermal springs.

COMPONENTS OF THE THERMAL FAUNA

The present day fauna we may regard either from the standpoint of the temperature and other characteristics of the water or on the basis of the origin of

its several components. Vouk ('23) has grouped the thermal fauna and flora in three divisions in relation to temperature, as follows:

Hypothermophilous Formations, below 15°C. (58° Fahr.).

Mesothermophilous Formations, 15°–30°C. (58°–86° Fahr.).

Euthermophilous Formations, 30°–80°C. (86°–176° Fahr.)

This classification is essentially arbitrary and artificial as it fails to recognize the varying critical temperatures for different types of organisms, although in its gradually rising scale, it necessarily reflects to some extent the progress of adaptation. From what has already been said concerning the temperatures withstood by unacclimatized animals of most kinds it is evident that there is a clearly marked point in the neighborhood of 40°C. (104° Fahr.) which is highly significant as it is the upper limit for all but the most resistant animals. Among plants on the other hand as already mentioned temperatures in the neighborhood of 60°C. (140° Fahr.) are critical as they mark the upper limits for chlorophyll-bearing vegetation.

From the standpoint of origin, Issel ('06) recognized five groups of animals in thermal springs:

1. Animals derived from cold water, usually species which are much more abundant and widespread in various other environments, and consequently with a greater ability to accustom themselves to extreme conditions.

2. Animals that come periodically from the sea or which migrate for short periods into fresh water from the sea.

3. Animals that, although absent in the ordinary waters of the region, occur in that of warmer regions.

4. Animals at present restricted to the thermal waters of a region, but similar to fossil species of much wider distribution in deposits from ordinary water in the same region.

5. Animals which are so far as known apparently restricted to thermal waters.

This classification is based on the probable evolutionary development of thermophily. The most recent accessions to the fauna are placed in the first category, and those types which are undoubtedly the oldest are placed last; the arrangement thus conforms to the probable origin and development of the thermal fauna as we have outlined them above. In many hot spring areas there is no access for animals from the sea, and the distinction between the fourth and fifth category is rather arbitrary. So far as category three is concerned this is an example of the fact that animals requiring moderate winter temperatures extend further toward polar regions wherever local conditions mitigate the rigors of the cold season.

As mentioned on a previous page, the most obvious characteristics of the thermal fauna are its evident origin from that of cooler water and its accession of a preponderance of types that are well represented in brackish or alkaline bodies of water.

INSECTS IN HOT SPRINGS

Many insects occur in hot springs, and these belong to a number of diverse groups. Numerous representatives of several families of aquatic beetles are known from thermal waters, mainly of Europe and the United States. Indeed, the members of this order occur more generally and abundantly in thermal waters than those of any other group of insects, and moreover, there appear to be some species that are restricted to warm springs and able to thrive at somewhat higher temperatures than those generally suited for animal life.

Several years ago in Yellowstone Park the writer collected six species of beetles in water of about 90°–100° Fahr. Of these three have close relatives that occur

FIG. 6. WATER-BEETLE, *Tropisternus californicus*

Both larvæ and adults were taken in pools at 90°–100° Fahr. in Yellowstone Park.

in brackish water. In hot springs in Arizona, Schwarz ('14) reported two species living in water at 110°–115° Fahr., and two of these, *Hydroscapha* and *Bidessus*, are characteristic denizens of European hot springs. In adjacent cooler water (100° Fahr.) there were eight other water beetles, most of which were to be found in the neighborhood in waters of ordinary temperature. In Europe another species of *Hydroscapha* occurs in Italian hot springs at temperatures of 30°–46°C. (86°–115° Fahr.) according to Issel ('01; '10) who found two European species of *Bidessus* at 30°–45°C. (86°–113° Fahr.). From these and numerous other observations on beetles we see that some kinds are adapted to extremely warm water and that all are in general closely related to types that extend into other saline environments.

Among other insects, the Diptera show the most interesting relations to thermal waters. Several families are represented: midges (Chironomidæ), horse-flies (Tabanidæ), soldier flies (Stratiomyiidæ) and Ephydridæ. The midges, which are all truly aquatic in their larval stages, include a number of genera that are exclusively marine, or which are represented by one or several species in the shallow parts of the sea. The larvæ of several species of *Chironomus*, like the vertebrates and in common with a very few other invertebrates, are provided with the respiratory pigment, hæmoglobin. In these larvæ the hæmoglobin is dissolved in the blood, which becomes a brilliant red and has earned for them the name of "blood-worms." Blood-worms occur more or less commonly in mud beneath water, in water-lily roots and in the deep water of lakes, while some other chironomid larvæ occur in sea water even at considerable distances off shore. In Yellowstone Park I found one species (fig. 7) abundant in the mud of a shallow thermal pool at a

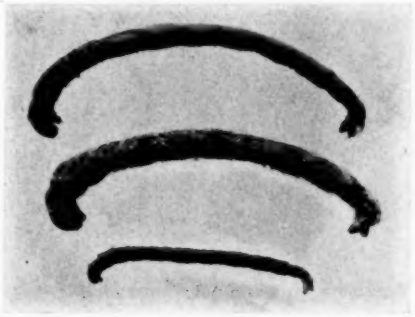

FIG. 7. "BLOOD-WORMS," *Chironomus*

These insects develop in mud, in lake water practically devoid of oxygen and in thermal water, sometimes at unusually high temperatures of 120°–124° Fahr.

temperature of 120° Fahr. (49°C.). As early as 1868 similar larvæ were reported by Owen from a Californian hot spring at 124° and more recently several others have found them in other American and European springs.

Leitch ('16) has studied experimentally the rôle of hæmoglobin in *Chironomus* and also in the snail, *Planorbis*, and comes to the conclusion that the function of hæmoglobin in these two animals, and among those invertebrates in general where it occurs, consists in making available, by its power of binding oxygen chemically, a quantity of oxygen sufficient for the needs of the animal at oxygen tensions so low that the necessary amount is not supplied by physical solution. He has shown also that the actual storage capacity for oxygen of the hæmoglobin in the larva of *Chironomus* is sufficient for only a few minutes of anaërobic life Thus hæmoglobin is characteristic of many mud-inhabiting invertebrates (van der Hyde, '22) that live where oxygen is present in only very small amounts. On the basis of Leitch's data we may thus interpret the ability of the larvæ of *Chironomus* to live under more or less completely anaërobic conditions, and we find them in the lower waters of lakes where the presence of oxygen cannot be detected by ordinary means. This is a common occurrence in fresh-water lakes (*cf.* also Immel, '16) and in thermal waters (Ziegelmayer, '24). In the alkaline water of silica-bearing hot springs

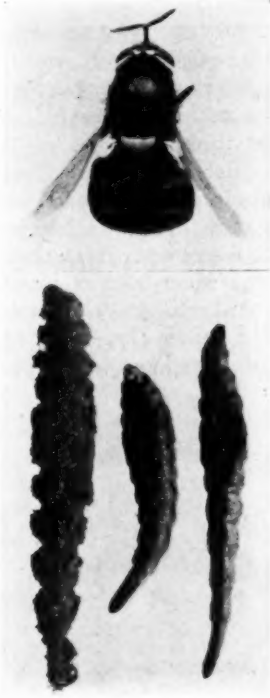

FIG. 8. SOLDIER-FLY, *Stratiomyia;* ADULT ABOVE, LARVÆ BELOW

The encrusted larva at the left has been overflowed by very hot water which killed it and deposited a heavy coating of calcareous material upon its body.

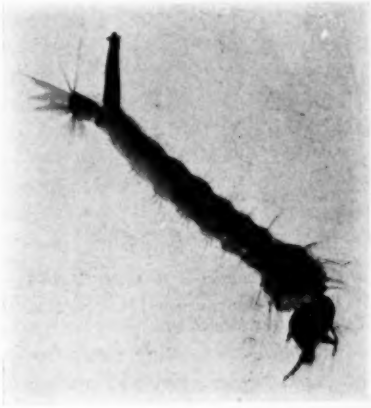

FIG. 9. MOSQUITO LARVA, *Culex tarsalis*, FROM SLIGHTLY WARM THERMAL WATER OF A TURBID POOL. YELLOWSTONE PARK

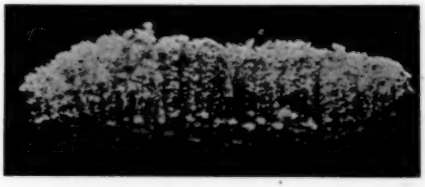

FIG. 10. CATERPILLAR IN THE PROCESS OF CRUDE FOSSILIZATION BY THE DEPOSIT OF CALCAREOUS MATERIAL ON ITS BODY. THIS UNHAPPY CREATURE FELL INTO WATER OF VERY HIGH TEMPERATURE

there is the added fact that among fishes at least, an increased alkalinity serves in some way to increase the resistance of these animals to a lack of oxygen (Packard, '05) and the same is probably true of *Chironomus* as the hæmoglobin appears to be identical in the two cases.

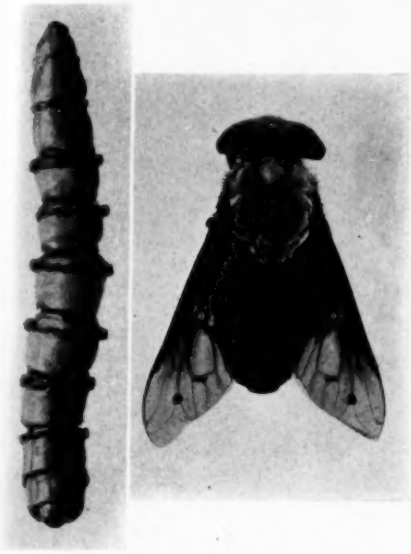

FIG. 11. HORSE-FLY, *Tabanus punctifer;* LARVA AT LEFT

The larva develops in alkaline lakes and in thermal water of moderate temperature. Taken in water at 91° Fahr. in Yellowstone Park.

The larvæ of stratiomyid flies have been found several times in thermal waters both in America and Europe and some are known to occur in the sea. Some belonging to the genus *Stratiomyia* (fig. 8) found by the writer along the edge of a hot streamlet in Yellowstone Park well illustrate the vicissitudes of life in such surroundings. These larvæ dwell in the algæ along the cooler edges of the stream, but when the latter changes its course, as frequently happens when mineral deposits clog its channel, the larvæ may be immersed in water hot enough to kill them. Deposits then cling to them and they become rapidly though somewhat crudely fossilized.

The larva of a large horse-fly, *Tabanus*, (fig. 11) occurs at temperatures up to about 100° Fahr. in the thermal waters of Yellowstone Park, where I have found them in several places. This same species (*T. punctifer*) breeds in alkaline water and related species develop in salt-marshes, as for example the common "green head" of the Atlantic seashore.

FIG. 12. BRINE-FLY, *Ephydra*

The larvæ of members of this genus develop in brine pools, salt lakes, alkaline ponds and warm springs.

Perhaps the most remarkable Diptera to be mentioned are the larvæ of *Ephydra*, small flies (fig. 12) which develop in saline pools, even in the extremely salty water of the Great Salt Lake. There are numerous species, some of which are unable to develop in fresh water, and one has been found in warm springs in Yellowstone Park.

Several other orders of insects are repre-

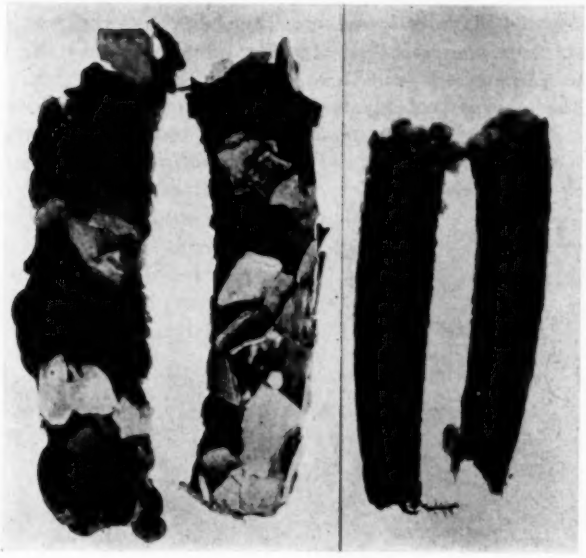

FIG. 13. CADDIS-WORM CASES

The larvæ of certain caddis-flies occur in thermal water. The photograph shows two forms from a warm sulphurous pool in Yellowstone Park. Attached to one is a small snail shell.

FIG. 14. WATER-BUG, *Ambrysus heidemanni*, FROM SILICA CHARGED WATER AT 96° FAHR. YELLOWSTONE PARK

FIG. 15. AMPHIPOD CRUSTACEAN, *Gammarus limnæus*. THE HABITAT OF THIS SPECIES RANGES FROM WARM SPRINGS TO ICE COLD ARCTIC PONDS

sented less abundantly in the thermal fauna, including the preparatory stages of

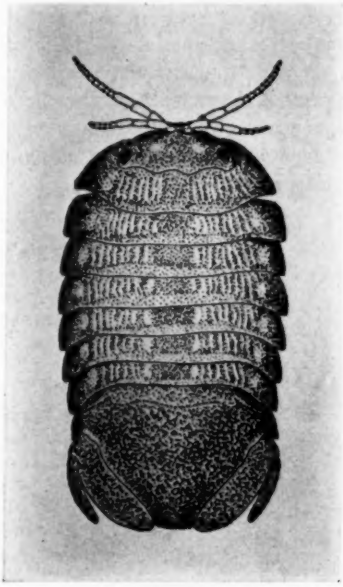

FIG. 16. ISOPOD CRUSTACEAN, *Exosphæroma thermophilum* FROM A NEW MEXICAN HOT SPRING (AFTER RICHARDSON)

Fossil species of a related genus are known to have inhabited hot springs in Oligocene times.

FIG. 17. SNAILS, A DWARF FORM OF *Physa heterostropha* FROM SILICA CHARGED WATER AT 96° FAHR. YELLOWSTONE PARK

certain caddice-flies (fig. 13), dragon-flies, may-flies and water-bugs (fig. 14).

CRUSTACEA IN HOT SPRINGS

Even a few crustaceans have adopted a thermal habitat. One interesting example is the small amphipod, *Gammarus limnæus* (fig. 15), which occurs in Yellowstone Park, has been found in a warm stream in Alaska and is widely distributed in cold ponds throughout northern America. It has even been found beneath two metres of ice during June in the far north, indicating a most remarkable tolerance to temperature. A few other crustaceans,

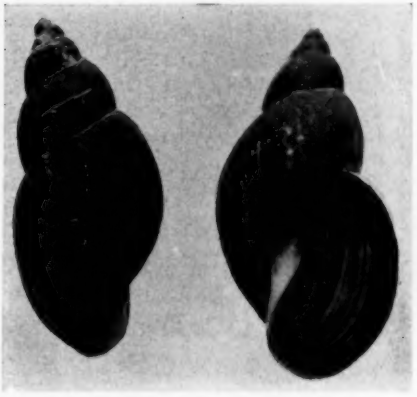

FIG. 18. POND-SNAILS, *Lymnæa palustris*, FROM WARM SULPHUROUS POOL FED BY HOT SPRING. WATER AT 80°–96° FAHR. YELLOWSTONE PARK

such as the copepod, *Cyclops*, and an isopod, *Exosphæroma* (fig. 16), are known from hot springs. The latter is especially interesting, as a closely related extinct genus is preserved in hot-spring deposits of lower Oligocene age, indicating a long thermal ancestry.

MOLLUSCS IN HOT SPRINGS

Many molluscs, mostly representatives of genera well represented in fresh-waters, are known to inhabit hot springs. Forms of the common *Physa heterostropha* (fig. 17) occur in Virginia, Utah and Yellowstone

Park, and several forms of a related species occur in European hot springs. These snails extend to water of about 106° Fahr. (41°C.). Several species of pond-snails, *Lymnæa* (fig. 18), live in thermal waters in Europe, Iceland and America, invading water of rather high temperature (37°–45°C.). One of these European forms apparently cannot live except in water above 27°C.

the writer in Yellowstone Park (fig. 19). There he found the tadpoles of a frog (*Rana*, probably *R. temporaria pretiosa*) in water ranging between 104°–106° Fahr. This is several degrees above the temperature at which unacclimatized frog and toad tadpoles succumb to the effects of heat, and seems to demonstrate unequivocally that acclimatization of amphibians may take place.

FIG. 19. OVERFLOW FROM HOT SPRING NEAR YELLOWSTONE LAKE. SNAILS AND TADPOLES INHABIT THIS WARM STREAM

VERTEBRATES IN HOT SPRINGS

As mentioned on a previous page, certain vertebrates must be counted as members of the thermal fauna. A few fish live in hot springs or streams, usually remaining away from the surface of the latter, where the temperature is higher. They are rarely seen in water over 25°–30°C. and apparently none can survive temperatures of more than about 40°C. (104° Fahr.) even for short periods.

An interesting case of adaptation to thermal water by an amphibian was noted by

GENERAL CONCLUSIONS

Many additional cases relating to diverse animals might be cited, but the foregoing summary is sufficient to illustrate the variety of the thermal fauna. It shows also that this fauna is of secondary rather than of primordial origin. The absence of typically marine kinds indicates that it has arisen mainly through the migration of forms from fresh water. Furthermore, the preponderance of species related to ones that have migrated into a marine or semi-marine (brackish) environ-

ment indicates that thermal and saline situations have imposed similar obstacles to the biota which has entered them from fresh water. These depend undoubtedly upon the presence of salts in solution and the attendant rise in the osmotic pressure of the medium. High temperature is a deterrent that has been overcome by acclimatization, ordinarily, however, within quite narrow limits, especially in the case of animals, which are able to endure much less heat than plants. An added inconvenience is the rather consistent dearth of dissolved oxygen in thermal waters, which renders respiration more difficult for purely aquatic animals.

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THE LOCALIZATION OF DEVELOPMENT FACTORS

By A. BRACHET

University of Brussels

THE PLACE OF EMBRYOLOGY IN GENERAL BIOLOGY

EMBRYOLOGY is the science that studies the embryological development of living beings and endeavors to determine its causes and mechanisms. Owing to these goals it holds an important place in general biology, and the first section of this article will be devoted to the precise statement of this place.

Life is, according to all evidence, a continuous phenomenon; it has proceeded, uninterrupted ever since its appearance on earth in remote geological times. Never, in fact, has the spontaneous generation of living beings been observed, and no man of science now has any doubts regarding the non-existence of this phenomenon. Life always appears to the observer or to the experimentalist in the shape of simple or complex living beings; to the continuity of life is opposed the discontinuity of living beings, of the organisms themselves. For, as a general rule, every organism is called into existence, goes through an adult state, then dies; exemption from death of some lower organisms has not yet been demonstrated, although some recent investigations make it seem at least likely. (Calkins, Woodruff, etc.)

In reality, in most cases—we may even say, in all animals—continuity of life is secured by the phenomenon of reproduction; we mean that every being before dying or while dying gives off germs able to reconstitute a being similar to the one from which it came and to perpetuate in that way the species to which it belongs.

Reproduction in animals—we shall not give our attention to plants where the facts are essentially the same—may doubtless assume various shapes, but sexual reproduction prevails above all, for it involves the entire problem of the actual origin of living beings. Agamous reproduction, viewed in all its bearings, is but one of its corollaries; and parthenogenesis, the development of an egg without the intervention of a male element, is either relatively uncommon or but a precious laboratory technique used to analyze fecundation; in any case it raises the same general question as sexual reproduction.

In every generation, every time a new organism is brought to life, the biologically fundamental question is put: what are the factors by whose action a fecundated egg, a zygote—whose structure is morphologically that of a simple cell—can, by a series of transformations and differentiations accompanied by growth, become a new being, identical in all its characters with all the representatives of the species to which it belongs.

The problem, viewed in a concrete way, may be stated as follows: the fecundated egg is a cell; as such it lives and displays all the physiological properties of living matter: it breathes, it feeds, it divides, it grows, etc. But it has also a peculiar physiological property: the natural and unavoidable consequence of its vital ac-

tivity is to give birth to a new organism. Now this particular property of the egg must have its substratum in its own substances although it cannot be anatomically perceived, and it is necessary to seek in that egg for its localization, its constitution, its real properties.

Every egg when it is fecundated is a complete cell and anatomically it is nothing else. It possesses a nucleus, whose chromosomes have been contributed half by itself and half by the spermatozoon, which is exactly like that of any normal cell of the species. It possesses a large cytoplasmic body laden to a various extent with nutritive matter, mitochondria, a Golgi apparatus and a centrosome which comes probably from the spermatozoon. I cannot here describe the evolution it has followed to reach this state,—its ripening, the ripening of the spermatozoon, fecundation—in spite of the great interest of these processes (see the book by E. B. Wilson). It is enough for us to take as a basis the fecundated egg ready to develop, that needs for this purpose only the usual exterior conditions: temperature, humidity, oxygen, etc.

Now a simple observation of facts, without using special experimental technique, has shown long since (Whitman, Rabl, Van Beneden, Julin, Robert, Conklin, etc.) with a great number of species already studied intensively (molluscs, annelides, echinoderms, chordates, vertebrates) that the egg, when beginning development, divides into two blastomeres, whose destiny is determined by the place they occupy in the whole; each of them depending on its position will give birth, by its subsequent evolution, to a determined part of the embryo's body and consequently, of the future organism. Segmentation is then a dividing up of the egg into a mosaic of blastomeres each one of which has its own potentialities and contributes a fixed part to the constitution of the whole body.

SEGMENTATION OF THE EGG

I should like to state precisely this purely descriptive notion with the aid of a few concrete examples, because these will be useful in our discussion, and because they will put in all its fullness the question of germinal localization in the egg. I shall take these examples from among the chordates because facts are clearer there, and also because with them have been carried out most of the latest researches that we shall analyze and that will settle the actual state of the question of the localization of developmental factors.

In the case of the tunicates (Van Beneden, Julin, Conklin) it has been clearly proved that the first segmentation plane cuts the egg into two equal blastomeres: we can follow the further development of these to see that they become, respectively, the right and the left half of the larva. Then the second plane, which is perpendicular to the first one and which cuts in two each of the blastomeres, divides the two half-embryos into two regions with definite potentialities and so on. Conklin especially has been able to follow with remarkable accuracy the cell-lineage of the tunicates in the case of *Cynthia*. The purely descriptive observations have long since received conclusive experimental confirmation (Chabry, Conklin): if we destroy, by means of a puncture, one of the first two blastomeres, the surviving one develops and gives birth to a right or left half-embryo. We can infer from these facts that the fecundated egg of the tunicates has a bilateral symmetry; it is made of two halves which the first segmentation plane separates and which have

THE LOCALIZATION OF DEVELOPMENT FACTORS

By A. BRACHET

University of Brussels

THE PLACE OF EMBRYOLOGY IN GENERAL BIOLOGY

EMBRYOLOGY is the science that studies the embryological development of living beings and endeavors to determine its causes and mechanisms. Owing to these goals it holds an important place in general biology, and the first section of this article will be devoted to the precise statement of this place.

Life is, according to all evidence, a continuous phenomenon; it has proceeded, uninterrupted ever since its appearance on earth in remote geological times. Never, in fact, has the spontaneous generation of living beings been observed, and no man of science now has any doubts regarding the non-existence of this phenomenon. Life always appears to the observer or to the experimentalist in the shape of simple or complex living beings; to the continuity of life is opposed the discontinuity of living beings, of the organisms themselves. For, as a general rule, every organism is called into existence, goes through an adult state, then dies; exemption from death of some lower organisms has not yet been demonstrated, although some recent investigations make it seem at least likely. (Calkins, Woodruff, etc.)

In reality, in most cases—we may even say, in all animals—continuity of life is secured by the phenomenon of reproduction; we mean that every being before dying or while dying gives off germs able to reconstitute a being similar to the one from which it came and to perpetuate in that way the species to which it belongs.

Reproduction in animals—we shall not give our attention to plants where the facts are essentially the same—may doubtless assume various shapes, but sexual reproduction prevails above all, for it involves the entire problem of the actual origin of living beings. Agamous reproduction, viewed in all its bearings, is but one of its corollaries; and parthenogenesis, the development of an egg without the intervention of a male element, is either relatively uncommon or but a precious laboratory technique used to analyze fecundation; in any case it raises the same general question as sexual reproduction.

In every generation, every time a new organism is brought to life, the biologically fundamental question is put: what are the factors by whose action a fecundated egg, a zygote—whose structure is morphologically that of a simple cell—can, by a series of transformations and differentiations accompanied by growth, become a new being, identical in all its characters with all the representatives of the species to which it belongs.

The problem, viewed in a concrete way, may be stated as follows: the fecundated egg is a cell; as such it lives and displays all the physiological properties of living matter: it breathes, it feeds, it divides, it grows, etc. But it has also a peculiar physiological property: the natural and unavoidable consequence of its vital ac-

tivity is to give birth to a new organism. Now this particular property of the egg must have its substratum in its own substances although it cannot be anatomically perceived, and it is necessary to seek in that egg for its localization, its constitution, its real properties.

Every egg when it is fecundated is a complete cell and anatomically it is nothing else. It possesses a nucleus, whose chromosomes have been contributed half by itself and half by the spermatozoon, which is exactly like that of any normal cell of the species. It possesses a large cytoplasmic body laden to a various extent with nutritive matter, mitochondria, a Golgi apparatus and a centrosome which comes probably from the spermatozoon. I cannot here describe the evolution it has followed to reach this state,—its ripening, the ripening of the spermatozoon, fecundation—in spite of the great interest of these processes (see the book by E. B. Wilson). It is enough for us to take as a basis the fecundated egg ready to develop, that needs for this purpose only the usual exterior conditions: temperature, humidity, oxygen, etc.

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their destiny fixed by their own constitution.

The egg of the frog behaves in the same way but with even more interesting variations. Long ago, Roux, after various observations especially involving destruction by puncture of one of the first two blastomeres in the egg of *Rana fusca*, concluded that these two blastomeres are one right and the other left and that they behave as in the tunicates; in consequence the first segmentation plane corresponds to the plane of bilateral symmetry of the gastrula and the future embryo. This conclusion of Roux and the deep speculative considerations that he drew from it initiated a great number of investigations, some confirming Roux's observations and others contradicting them (Hertwig and others). This controversy is now over; it has been very useful because it put the question of the germinal localization of the egg in its proper place, and gave it the form that it actually has at the present time.

A long series of researches by Roux, Morgan, Schultze, Brachet, etc., has settled, in the particular case of *R. fusca*, a series of very interesting facts, which allow important comparisons to be made with other nearly or distantly related species. I shall give a résumé of them with a few details, but without describing completely the methods used to discover them—that would lengthen this article beyond all measure.

SEGMENTATION OF THE FROG'S EGG

If we examine living frogs' eggs (*R. fusca*) fecundated two or two and one-half hours previously we see that the brown pigment that covers with a compact layer the whole upper hemisphere does not stop under the equator along a regular latitudinal line. In one half of the egg it goes far down, often covering half the distance between the equator and the lower pole; in the other half, the compact pigment scarcely reaches the equator, and between it and the whitish polar regions is a line of gradual transition, assuming the shape of a crescent whose grayish color has won for it the name of gray crescent (fig. 1). The median part of the crescent (its largest) reaches the equator at its upper edge; its horns spread right and left, and below it disappears in-

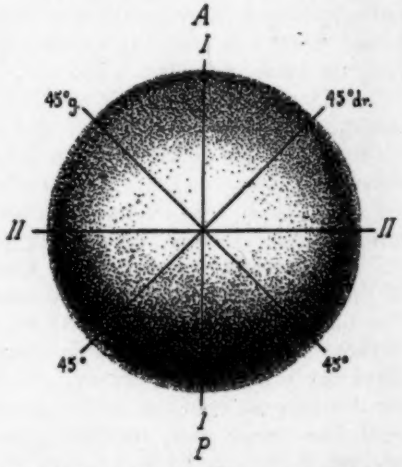

FIG. 1. EGG OF *Rana fusca* SEEN FROM THE LOWER HEMISPHERE SHOWING THE GRAY CRESCENT IN THE UPPER HALF

I-I, plane of bilateral symmetry. II-II, plane perpendicular to the plane of symmetry. 45°dr., 45°g., oblique planes where the first cleavage furrows occur.

sensibly. The gray crescent does not exist in the virgin egg; it becomes visible only after fecundation or after stimulation by one of the agents of experimental parthenogenesis that Bataillon has made known (Brachet). Evidently (fig. 1) the presence of the gray crescent allows us to trace on the egg a vertical plane (from the upper to the lower pole) which divides the gray crescent into two halves and which is consequently a plane of bilateral symmetry; that is to say, it makes out-

wardly visible the bilaterally symmetrical constitution of the fecundated egg. These characters added to its clearness, which is very good in some cases, make it a precious guiding mark for descriptive and experimental analysis.

Now we may consider as perfectly proved that this plane of symmetry of the fecundated egg always becomes the plane of symmetry of the gastrula and the embryo; and the egg is consequently made of a right and a left half which contain in themselves the determinative factors for the formation of the right and left halves of the body.

When segmentation begins, the first cleavage furrow often coincides with the plane of symmetry of the egg and a right and a left blastomere are formed; if we kill one of them, thus reproducing Roux's fundamental experiment, we get a right or a left half-embryo out of the still living blastomere. But it is not always so (Morgan and Boring, Brachet, etc.). Very often the first segmentation plane is oblique to the plane of symmetry. Then the two blastomeres will not have an equal share of the gray crescent substance; one of them will hold, according to the direction and the extent of the obliquity, a whole half and also a more or less big part of the other half (fig. 1). The two blastomeres are then not identical in this respect, although their bulk is the same; and experience shows also that their potentialities are different; the destruction of the one does not cause the other one to develop into a half-embryo.

To sum up the results of the experiments, we may say that the quantity of dorsal organs (central nervous system, dorsal chord, mesoblastic somites) derived from a blastomere in a case of oblique division depends on the quantity of material from the gray crescent contained in it according to the position of the first segmentation plane. These ideas will be more precisely stated later; but they allow us already to draw an important conclusion: in the egg of *R. fusca*, the potentiality of the first two blastomeres is not fixed; it may vary within very wide limits. But it may be foreseen, since we can mark exactly the direction of the plane that divides them in relation to the plane of bilateral symmetry which is fixed and immovable.

But whatever is the direction of the first segmentation plane, be it regular, oblique, or perpendicular, the second plane that follows it and that divides the egg into four blastomeres, is always at right angles with it. If then, the potentiality of the first two blastomeres is changeable for reasons already seen, the potentiality of the first four will also be changeable for the same reasons and so on for the further stages of segmentation. Segmentation is consequently but a cutting up of a fecundated egg into cellular regions; it leaves its composition and germinal localizations untouched and this condition continues until the localized substances begin to display their own potentialities. Since the gray crescent is only a part of the cytoplasm of the egg, and since it determines the destiny of the various regions of the egg, it follows that the first determination does not occur in the nucleus, but instead appears in the cytoplasmic composition of the egg-cell.

LOCALIZATION WITHIN THE EGG

These points being settled, we can go further and try to localize in the egg, the regions that will give the different organs of the embryo's body. For this also descriptive and experimental observations of the frog's egg have given very clear results, which is partly due to the fact that the gray crescent remains more or less visible until an advanced stage in segmentation. It can still be easily recog-

nized in the blastula in some favorable cases.

We have been able to recognize, by various methods, the following facts derived from the observations of a great number of authors (Roux, O. Hertwig, Morgan, Kopsch, Brachet, etc.) and they follow the lateral horns of the gray crescent and finally join each other in a caudal lip on the opposite half of the egg, But while this large blastopore is developed, the parts first formed concentrate towards the median plane, growing closer to the caudal lip by a concentric motion.

FIG. 2. YOUNG GASTRULA OF *Rana fusca* SEEN FROM THE LOWER HEMISPHERE

Comparison with figure 1 shows that the blastopore shaped like a horse shoe is formed in the region of the gray crescent.

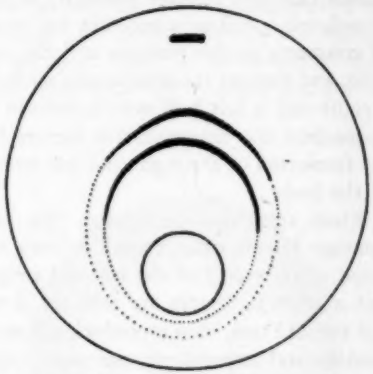

FIG. 3. SCHEME OF THE CLOSURE OF THE BLASTOPORE IN THE FROG
(After Morgan)

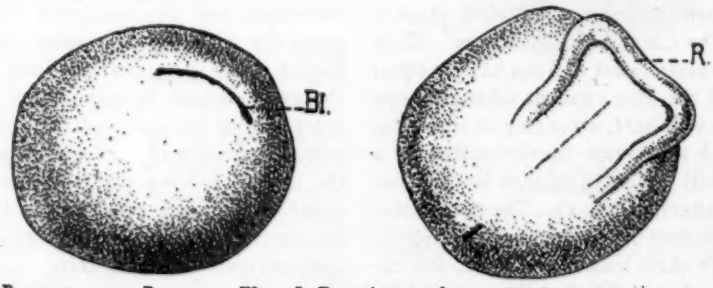

FIG. 4. POSITIONS OF THE BLASTOPORE WHEN IT FIRST APPEARS (AT THE LEFT) AND WHEN ALMOST CLOSED (AT THE RIGHT)

In the space between these two positions the medullary plate is formed. *R*, transverse cerebral fold. (After O. Hertwig.)

recently confirmed by Bataillon, by means of a very clever process: when in *Rana*, gastrulation begins, the position of the blastoporal lip, which appears first—the cranial lip—is found in the larger part of the gray crescent, that is to say, at a small distance under the equator (fig. 2); and as soon as its lateral lips are formed Figure 3, supplied by T. H. Morgan, shows the reasons for this evolution. All the materials of the blastoporal lips taken together constitute a plate which occupies the same area as the blastopore and that may be called the dorsal plate. Indeed it is in that region that the medullary plate, rough cast of the central nervous

system, the dorsal chord, and on both sides the mesoblastic somites from which the myotomes will come, will be differentiated. The anterior portion of the medullary plate, called by embryologists the transverse cerebral fold (fig. 4), which will give rise to the prechordal region of the brain (optical and olfactory regions), will be formed just above the spot where the cranial lip of the blastopore appeared first. The caudal lip of the blastopore will become the anal region.

All this development is naturally accomplished by means of complicated processes concerning whose nature there is as yet no accord; there are emigrations and displacements of material which have just been made known by the works of W. Vogt. But it is not necessary for us to enter into these details: the essential point is, for us, that in *Rana fusca*, the gray crescent, and perhaps the adjacent parts, are the starting point of the processes thanks to which the embryo's dorsal organs, characteristic of every chordate, are formed. The further growth of those organs, their displacements, etc., are secondary facts that will not be discussed in this article. Let us add, to complete the subject, that if the localization of the dorsal organs is determined that of the ventral organs is also determined.

The exact topographic details made known in this article are only valid for *R. fusca;* they may be somewhat different in the other Anura and we shall see that numerous researches, especially by Eycleshymer, Spemann and his pupils, show undoubtedly that they are different in some important points for the urodele amphibians.

In other groups exact localizations have been demonstrated, as we have already said. Especially in the tunicates (that are also chordates) the work of Conklin, to which Duesberg added some interesting facts, made it possible, because of certain details of structure and appearance, to localize the formation regions of axial organs more accurately even than in the frog.

In other animals the existence of the region of a fecundated or segmentated egg, where the developmental factors of the larval organs are localized has been settled for a long time, and everybody knows the fundamental observations on this point of E. B. Wilson on the importance of the polar lobe of the *Dentalium* egg. It is the same also with the ctenophores and even with others where we should least expect it, as with the echinoderms. In reality, we can say that the existence of germinal localizations in the egg or in the blastula is a quite general phenomenon.

The constant existence of localization of developmental factors is certainly a valuable idea, but as we have considered it hitherto it has only a descriptive value, although it has been made obvious by experimental methods; it is a kind of map of evolutionary potentialities, and is only a starting point for a deeper analysis of the real factors of embryological development. The most recent works are devoted to this analysis, as are also the following pages. The first questions are those of the origin, and of the moment and order of appearance of germinal localizations. We are still here in a very dark region and have only scattered information that cannot yet be generalized.

THE RÔLE OF EGG AND SPERMATOZOON

In the actual state of science, we can only determine the real power of a germ when it is allowed to develop, that is, to realize its potentialities. From our standpoint the fundamental property of the egg—one of the greatest riddles of biology—to give birth to a new organism by har-

moniously linked and strictly localized processes, must be submitted to analysis by means of the following questions: (1) what is the rôle of the egg and the spermatozoon respectively in determining localizations; (2) are localized materials formed quickly, abruptly or do they arise slowly and progressively, and: (3) are they immutable or do they change under certain circumstances?

In trying to answer these questions we shall leave the descriptive account of germinal localizations and proceed to the analysis of their nature and real meaning.

Concerning the first question we have already a certain number of interesting and demonstrated facts. It is evident that the egg has in itself everything necessary for complete normal development; for not only are there eggs that have a normal parthenogenetic development, but a great number of others that usually develop after being fertilized by a spermatozoon, may develop by parthenogenesis, if stimulated by some physical agent.

The question is more complex with respect to the spermatozoon; we have a tendency to admit on principle that sexes are equivalent. If this were really true, the spermatozoon, which is but a nucleus surrounded by very little cytoplasm, would also contain the basis of all developmental factors, and the enormous bulk of the egg cytoplasm with all its inclusions would only be auxiliary, particularly nutritive matter, the destiny of which would be entirely directed by the nucleus and the small protoplasmic region that surrounds it. All this is quite likely, but experience must determine whether it is true.

A great number of experiments have been tried with this in mind. Some authors (Yves Delage) thought they had stimulated a "male parthenogenesis" but their results did not withstand analysis. In any case, the spermatozoon acted as an agent of cellular division and could change in only a few points the fate of the cytoplasm into which it had been introduced. I cannot enter into detail concerning those experiments which I have already criticized (Brachet, 1917), but I do not know a surer proof of the limitation of the spermatozoon's potentiality than the results of polyspermy experimentally realized in the egg of *Rana fusca* (Brachet). When this polyspermy is not very pronounced, it brings about a remarkable result. In the voluminous frog's egg, when 5 or 6

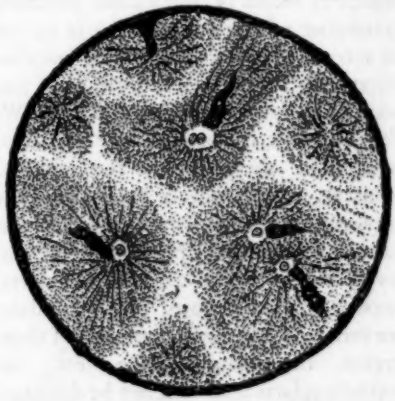

FIG. 5. SECTION OF A POLYSPERMIC EGG OF *Rana fusca* SHOWING MANY SPERMATIC ENERGIDS; IN ONE OF THEM THE PRONUCLEI ARE UNITING

spermatozoa enter simultaneously, but in spots that are rather distant from one another on the upper hemisphere, each one forms in the nearby cytoplasm a spermatic aster and produces what may be called a spermatic energid. Each of these is well separated from its neighbors by clear bands (fig. 5). Each one has a nucleus and a centrosome; one of them, the one that lies at the upper pole, appropriates the egg nucleus (female pronucleus) which unites with that of the spermatozoon. When segmentation begins, in each energid a segmentation

mitosis takes place, exclusively spermatic in all except the one containing the female pronucleus. The egg divides simultaneously into as many blastomeres as there were spermatozoa that entered the egg (fig. 6). Each of these blastomeres has, it is true, two nuclei and two centrosomes, but as segmentation proceeds, a process of regulation takes place and finally all the cells have only one nucleus. This kind of egg develops quite well, produces a normal larva which hatches and may live for some time; such larvae always die,—

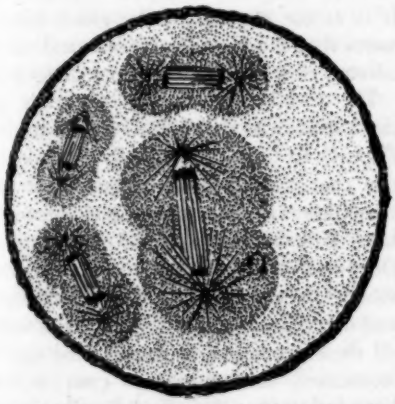

FIG. 6. SECTION OF A POLYSPERMIC EGG OF *Rana fusca*
Each energid contains an amphiaster

for reasons already analyzed and that have no interest from our standpoint.

Here is then a pentaspermic egg in which 4 spermatozoa occupy a large area where they stimulate division and development without any association with the female nucleus. If the spermatozoa had the necessary potentiality to produce an embryo they would, in such an egg, be in an ideal situation to do so, and if those potentialities were even partially realized the egg would become a manifold monster. This does not occur; in the best of cases the egg develops quite normally, the gastrulation is typical and all the embryonic organs form in the normal way. The inevitable conclusion is then, that the destiny of the various egg regions is fixed by their own composition and cannot be changed by the action of a spermatozoon, or, on the other hand, the destiny of a spermatozoon is absolutely dependent on the composition and properties of the ovular cytoplasm in which it has become entangled and the development of which it only helps to initiate. This does not mean that in a normal fecundation the spermatozoon plays no part in the establishment of germinal localizations—we shall later see a proof of that—but it is unable to create them.

This point being settled, the problem becomes clearer and it is in the egg that we must look, first of all, for the primary origin of developmental factors. Let us first see exactly what we know on this point; we shall then indicate the lacunae in our knowledge and what we can expect from further research work.

THE ORIGIN OF DEVELOPMENTAL FACTORS IN THE EGG

In the first place, there are purely descriptive observations, although these are very instructive, made on eggs in which the localization of materials forming the embryo's important primary organs is outwardly visible even before the beginning of segmentation and appears progressively during ripening and fecundation.

A very obvious case is that of the ascidians, especially of *Cynthia partita*, made known by Conklin. During the ejection of polar globules that ends maturation, and the penetration of the spermatozoon, an altering of the cytoplasm takes place; two crescent-shaped, differently colored bands that appear on the surface, superposed and localized in one half of the egg, show externally a

bilateral symmetry which will be the symmetry of the future larva; these enable us to localize exactly the ovular material out of which will progressively be formed the nervous system, the dorsal chord and the mesoblast. We can say that those two bands represent these organs potentially.

Another rather similar case, long studied by Roux, O. Schultze, Morgan, etc., is that of *Rana fusca* whose egg is one of those preferred by experimental embryologists. At the moment of deposition the egg is in the anaphases of the formation of the first polar body. It is then fecundated and, 2 or 2½ hours afterwards, the gray crescent of which we have already spoken and which represents also potentially the embryo's dorsal organs, (nervous system, dorsal chord, mesoblastic somites) appears little by little in one half of the egg. This gives to the egg a bilateral symmetry, the vertical plane of which may easily be identified on the living egg; this is also the plane of bilateral symmetry of the gastrula and the embryo (figs. 1, 2, 4).

In the two instances noted here because they are very characteristic, the outward appearance of germinal localizations and consequently of bilateral symmetry, and the definitive localization of cytoplasmic material coincides with maturation and fecundation.

But there are some cases in which, by observation alone, we can see that the localization of the principal materials is much slower. This is particularly true of a ctenophore, *Beroë ovata*, concerning which Spek (1926) has published a very interesting account. The fecundated egg is covered on all its surface with a coating of hyalin cytoplasm which through the ultramicroscope has a beautiful green hue. From the beginning of segmentation until the 8-cell stage, at every division the green substance enters the cleavage furrow, then when the furrow is completed, it spreads again uniformly over the new blastomeres; its fluidity allows these consecutive movements. But after the 8-cell stage its viscosity increases; when the 8 blastomeres are fully formed the green substance instead of spreading over their entire surface, remains heaped in a determined spot in each of them and so takes a definite position. At the next division, the green substance is nearly all retained by the 8 micromeres, the macromeres keeping only a very small part of it. It is at the expense of the green micromeres that the larva's epidermis and the 8 ciliated ribs of the ctenophore develop.

The localization of the materials of those structures, that helped (cf. especially Fischel) to prove the strictly determinative character of the development of the ctenophore is then slow, and proceeds long after maturation and fecundation. Of course, it is important to note that the substances embodied in the micromeres and determining their destiny, exist with all their properties as soon as the egg is fecundated and probably even before fecundati on; but their final distribution is only completed during segmentation. The substance of the colored crescents in the tunicates and that of the gray crescent in the frog certainly exist also in the egg before maturation and fecundation, but they are scattered in another way, probably more uniformly; they are also localized more quickly and before segmentation begins. We can imagine cases in which localization settling is even slower than in the ctenophores or quicker than in *Rana* and *Cynthia*. This is true, for instance, with *Dentalium* (E. B. Wilson) where the virgin egg has around its lower pole a light region called by Wilson the polar lobe, which is very important from an organogenetic point of view.

EXPERIMENTS ON THE ESTABLISHMENT OF GERMINAL LOCALIZATIONS

This leads us to say a few words about experimental researches on the establishment of germinal localization. E. B. Wilson has stated that if we cut the unfertilized egg of *Dentalium* into two parts by an oblique plane, one of which includes only the polar lobe, the two parts may be fecundated, but the one including the polar lobe is the only one able to produce a trochophore; the other one undergoes only a partial development. There is then already in the egg, at this very early stage, a localization of formative energies. This localization becomes certainly more precise and complete toward the end of maturation and fecundation, but it has been nevertheless largely prepared at earlier stages.

The action of maturation and fecundation is clearer in *Cerebratulus*. In this kind of egg the cutting off of a part of the cytoplasm, before the ejection of the second polar body, does not prevent normal segmentation (Zeleny); if the cutting is done a little later, it causes more and more striking anomalies going even so far as a half segmentation, which is a typical partial segmentation. Even in the sea-urchin, Delage had already stated that if we take off any part of the cytoplasm of the ripe egg, not only is fecundation normal, but also segmentation; nothing seems changed with respect to the egg's potentialities. If, on the contrary, we separate the first two blastomeres from one another, each of them goes on dividing and produces a half-blastula which will be later completed by a secondary change (Driesch).

We could mention other similar instances but those that I have already chosen and that have long been known are sufficient to show that in every egg, between the beginning of maturation and the beginning of development, something happens which establishes in a final way, more or less completely, the substances and energies which will direct the harmony of development and which were until then more uniformly distributed in the egg. And we can foresee some cases in which stabilization is delayed and is only completed at a later stage of development.

We have not been able until now, to specify which of these very important phenomena give to the whole development the direction it must follow, which part is due to the maturation process, and which to fecundation, for most often these processes occur together. In *Rana fusca* we have been able to examine the facts very closely and so have come to some conclusions that are very interesting, although we are not yet able to generalize (Roux, Brachet). We have already seen that a fecundated egg of *Rana fusca*, 2 or 2½ hours after it is brought into contact with the sperm, has its germinal localization finally established and fixed, and that these show themselves outwardly by the appearance of the gray crescent on one half of the egg. Now we can state, by a simple anatomical examination corroborated by Roux's localized fecundation experiments, that the sperm's entrance spot, easily identified because of a trail of brown pigment, is situated in the upper hemisphere of the egg in the half opposite to the one occupied by the gray crescent, and also that this spot is always exactly in the meridian of bilateral symmetry of the egg, which cuts in two the gray crescent.

This constancy would lead us to believe in a predetermination of the entrance spot of the sperm and consequently of the plane of bilateral symmetry. Conklin thinks this is true of the tunicates, but several experiments here show that this is

not the case. These experiments are, first, Roux's "localized fecundations." We can by simply applying a fine silk thread on one of the meridians of the egg, and immersing it in water full of sperms, localize very exactly the entrance spot of the spermatozoa and Roux has stated that in more than 80 per cent of cases there is an exact correspondence between the silk thread meridian and the meridian of bilateral symmetry.

Another proof that looks final is that given by experimental polyspermy. I have shown that in the usually monospermic egg of *Rana* we can easily bring about a simultaneous penetration of several spermatozoa. We can accomplish this by using very concentrated sperm and by rendering the egg's reactions slower by the influence of a 2½ per 1000 NaCl solution for a few minutes. These spermatozoa enter by every possible meridian, we can thus see that there is not a preestablished place of entrance. Moreover, Herlant has observed that when two spermatozoa enter the egg simultaneously, the plane of bilateral symmetry, when it becomes visible, passes exactly between the two fecundation meridians. These proofs seem to settle the question.

But the frog's egg shows even more and allows us to proceed further in the fecundation phenomenon. This phenomenon, as has been stated already orients the germinal localizations. This is an important fact in fecundation, for which I have proposed the name "dynamic manifestation of fecundation." But, in the absence of any spermatozoa, the localization may be brought about by the egg itself. Indeed let us subject an unfertilized frog's egg to the first act of parthenogenesis by Bataillon's methods—puncture with a fine needle, induction shock, etc.—two or three hours afterwards, the gray crescent appears, normally shaped. When we operate with the needle, there is no connection between the plane of bilateral symmetry and the point of the puncture.

The final orientation of the germinal localization is then here purely an ovular phenomenon and it seems to come at the end of a long period of preparation. The egg, under certain experimental conditions, reacts by its own power and establishes its own germinal localization and bilateral symmetry. This shows that the symmetry existed already in the virgin egg but was vague and labile; in normal fecundation the spermatozoon is the determining factor.

It is interesting to remark here that some observations by Just allow us to extend to the echinoderms a great number of the conclusions obtained with the frog.

In the *Rana* egg we can add to the action of the spermatozoa some experimental data which show very precisely that the establishment of germinal localization proceeds with the course of the fecundation process. Experiments show, in fact, that if we take a frog's egg, that has already been in contact with sperm for 30 or 45 minutes, and destroy with a thin slightly heated needle a localized part of its substance, the egg begins to segment, eliminates the part destroyed and produces a perfectly normal embryo. The same experiment made ¼ hour later gives a different result; the region of the egg near the part destroyed does not develop as well as the rest and with a quantitative deficiency; the embryo is then asymmetrical although all the organs seem well shaped in it. If we can operate ¼ or ½ hour later (1¼ or 1½ hour after the addition of sperm) the result of the injury is the same as the destruction of a blastomere during segmentation; a partial embryo is formed lacking that part of the body that

would normally have come from the destroyed part of the egg (Brachet).

The influence of fecundation on the final establishment of germinal localization is shown by these experiments with sufficient evidence, so that it is useless to discuss the subject further.

MATURATION, FECUNDATION AND SEGMENTATION

But we must now settle the exact meaning of all the facts here related regarding the effects of maturation, fecundation and even segmentation (ctenophores) on the establishment of germinal localization. In reality they do not show us the beginning in the germ of the developmental factors and energies which will be used to build the organs and regions of the body, but simply their localization in some definite spot in the egg. It seems, if we examine our data carefully, as though dynamic and material developmental factors scattered more or less homogeneously in the unripe virgin egg, separate and dissociate the one from the other when ripening begins and even more when fecundation takes place, and settle in spots on the right and left side of a median plane, the normal destiny of which is fixed by their nature and composition.

This localization, whose physico-chemical determination has not yet been successfully studied, may be, according to the species, early, more or less late, speedy or slow, and also, as we shall see later, rigid or more or less labile. In the discussion I have presented here, I have been obliged, in order to give a clear account, to choose cases where this stability, when it is final, is great enough to give segmentation a strict determinative character so that we could assert, on positive facts, that the ontogenetic potentialities of the first blastomeres are strictly determined by the nature and quality of the materials and energies in the fecundated egg, given to them by the direction of the cleavage planes. We shall later speak of numerous cases where regeneration phenomena take place, and where a region of the egg may, under certain conditions, acquire greater morphogenetic potentialities than we would think possible by the quality of its germinal localizations.

In other words, each area of the germ, with its own germinal localizations, has a typical normal destiny, due to what we may call its real potentialities (this expression is equivalent to Driesch's "prospektive Bedeutung"). If with the help of experimental intervention it produces more than its normal destiny, it is because its real potentialities are only a part of a total potentiality ("prospektive Potenz" of Driesch) which will never appear in typical development.

We shall come back later to this point; it was necessary to mention it at this point, because these two kinds of potentiality are connected, by means of a tie not yet discovered by science, but existing nevertheless, with the same differences as stated in the frog, for instance, between the virgin and the fecundated egg; in the one a small loss of substance does not prevent a complete development; in the second, the same loss gives a partial development. We can express this fact by saying that the virgin egg having lost a part of its substance has a total potentiality which the fecundated egg no longer has.

The virgin egg's potentiality (the egg being identical in all its parts) is only due to the fact that the formative materials, the substratum of the developmental energies, are scattered in a more uniform way than in the fecundated egg; they are still more or less mixed and not yet localized in the spots where they will develop. The cutting off of a part of the

egg takes from it a small part of all the materials of future germinal localization and so the deficit is essentially quantitative; the formed embryo will be a little smaller but complete.

This is, at least, the most satisfactory explanation that we can give of these facts; it is simple and may be easily verified by well-conducted experiments. Everything shows, in fact, that the materials and energies of germinal localizations, mixed in the virgin egg, and later localized, have their seat in the egg's cytoplasm and not in the nucleus. When we take off the polar lobe of *Dentalium* or a part of the gray crescent of a frog's egg, the nucleus is untouched and the cytoplasm alone suffers a loss; and the consequences are well known.

THE ORIGIN OF FORMATIVE MATERIALS IN OOGENESIS

But analysis ought to be pushed further, for as I have already said, the question of the primary origin of the egg's formative materials, has not yet been touched in this essay; we have spoken of their removals, of their changes, of their localization which are, in fact, the first stages of ontogenesis. But we have only examined them when their existence was already exhibited by their own developmental properties.

When and under what influences do they arise? When we shall be able to answer this question with objective data, a great part of the enigma of the actual origin of living beings will be explained; but at present we cannot do so. We can only build up some hypothesis resting on presumptions, but nothing more.

The egg of any animal species, when ready to begin maturation,—which is shown by the breaking of its germinal vesicle previous to the forming of the amphiaster of the first polar body,—is the result of a long evolution in the ovary. It was first, as an oogonium, a small cell; it becomes an always large and sometimes enormous cell. During the growth period it is the seat of an active metabolism in which the cytoplasm as well as the nucleus plays a part. The nucleus undergoes at first very complex alterations in its chromatin; the chromosomes unite and prepare for reduction which will be fulfilled during maturation; then it swells, takes the appearance known by the name of germinatal vesicle which it will keep as long as the swelling lasts. Cytologists have minutely described these various phases which have tended to give to the chromosomes the importance they now have. But while these phases go on, the egg grows, its cytoplasm increases; inclusions and reserve materials accumulate; mitochondria increase in number, they shift their places, and take a share in the work of the egg cell. The growing phase of the oocyte is an elaboration period, during which the cytoplasm acquires its material composition, its physical and chemical state and consequently the energy sources which it will use to start development. But we do not know anything of what would interest us here, that is, of the preparations for germinal localizations, except that they are probably carried on at this time. Does the primeval impulsion to this preparation come from the nucleus, as all those authors maintain that localize in the nucleus all hereditary tendencies—which is synonymous of developmental factors—and as Gregoire has recently attempted to demonstrate by purely theoretical consideration? That may be; the nucleus is an active element in cellular metabolism, but the cytoplasm with all its constituents is none the less so, and we cannot say whether one of them prevails over the other.

Our ignorance regarding the physiology

of an important phase of the egg's development comes from an essential cause: the existence of germinal localizations in the egg can only be determined if we make the egg develop. We only know of their existence because we see them differentiate into organs and structures. But up to the present time we have never been able to make an egg develop when it had not yet completed its growth, that is, to a shape where all morphogenetic material is formed and only requires localizing. This localization, sometimes begun during maturation, always made more rapid by fecundation or parthenogenetic processes, in certain cases fulfilled by segmentation, which is in reality the beginning of organogenetic development, is it only possible when all materials have been prepared in the egg?

If we only inferred from what we know at present, we ought to answer in the affirmative; but the confidence that would follow might be premature. In reality, we have never tried to stimulate the development of an unripe oocyte; technical difficulties would be very great; the segmentations discovered by chance in mammalian ovaries (cf. Branca's recent work) are evidently abortive and are not very interesting from our point of view.

But nothing prevents us from hoping for the discovery of new techniques and the goal is important enough to make us look for them. It is, in fact, theoretically possible that the materials of the various germinal localizations are not elaborated simultaneously, but that there may be a kind of hierarchy, be it only of a chronological kind.

If this were so the egg's ontogenetic properties would not only grow uniformly but also increase in number during its growth. In the various stages, previous to the acquisition of its final shape and bulk, it might be different not only quantitatively but also qualitatively, because of the developmental factors it holds. If it were so, and if we could, at those stages, stimulate the development of the growing egg, and make its formative materials give all that they can give, we could get to a dissociation of normal development that would be very interesting and could win back a new interest for phylogenetic considerations resting on ontogenetic characters.

Perhaps none of these experiments may be realized, and any premature development may be impossible, for technical or practical reasons. The origin of germinal localizations can nevertheless be studied by other means: by the general and particular study of chemical composition and physical state of the egg's cytoplasm at various stages of its development. The germinal localizations finally established are only local variations in the composition of the egg's cytoplasm; these variations are as much of a quantitative as of a qualitative kind and consequently as much, and even more, of a physical as of a chemical nature.

We may believe and even hope that the remarkable investigations made these last few years on the physics and chemistry of the cell: cellular constants, viscosity, surface tensions, permeability, etc.—and their variations during vital activity (André Mayer, Schaeffer, Conklin, the Lillies, Heilbrunn, Chambers, Herlant, Dalcq, Vlés, Fauré-Frémiet, Spek, Runnstrom, etc.) may be used as an introduction to a study no longer of the whole but of every separate region of the egg at different stages of its evolution, and so chemistry and physics will help to explain some morphogenetic phenomena. It is a very interesting domain to explore but it belongs almost entirely to the future.

ONTOGENETIC PROPERTIES OF GERMINAL LOCALIZATIONS

In the meantime let us take up again established germinal localizations and let us try to go into the details of their ontogenetic properties; now that we have acknowledged their existence and their importance, it is interesting to know, as far as possible, under what laws their potentialities display themselves. Some important investigations have been published these last few years on this question, making known some very remarkable facts. They were particularly carried out on anuran and urodelan amphibians. Up to this point we have spoken only of the former, because the facts we wished to emphasize are, there, more evident and can be more easily demonstrated; it is now necessary to speak about the urodels which have some important peculiarities and even give to the experiment some possibilities that cannot be realized on the Anura.

Triton has for many years been a subject for the study of causal embryology. Herlitzka's observation has long been classical: he divided the first two blastomeres by means of a hair or silk thread and obtained two well developed twin embryos. In recent years considerable progress has been made owing to Spemann and his pupils (O. Mangold, Hilde Mangold, G. Ruud, Marx, Geinitz).

The essential facts from our point of view are as follows. There is not in the fecundated egg of *Triton* either a gray crescent or any sign that reveals to us, on superficial examination, a germinal localization or a plane of bilateral symmetry; the poles only are easily recognized. Experiments show, nevertheless, as Spemann and Ruud have proved, that these really exist. Indeed, if we separate the first two blastomeres one from the other by Herlitzka's method or any other method of Spemann's school, we obtain two very different kinds of results. Sometimes, each of the two blastomeres develops and gives a normal embryo, smaller of course, by half, and the two embryos only differ in certain details. At other times, one only of the two blastomeres gives a complete embryo; the other one forms a more or less vesicular mass containing neither nervous system nor chord nor myotomes, and consisting only of ventral elements of the body. We can infer that in the first case the first segmentation plane coincided with a plane of bilateral symmetry already existing in the fecundated egg but invisible; that the first two blastomeres are then a right and a left one as in the frog; but instead of giving each a lateral half embryo, an alteration takes place in their substance and each half embryo is completed by producing the half it lacked. This may be obtained also in the frog (Schultze, Morgan) by turning the eggs that are segmentated in two upside down, the white pole being above and the black pole below; the alteration in each blastomere, made necessary by the inversion of the direction of gravity, divides again the formative materials as they are in an egg and two twin larvae develop. These facts may be explained without much difficulty, for the right half of the body is not much different from the left and contains in itself everything that is necessary to produce all the organs in their normal place. It is sufficient to realize the right conditions for a new bilateral, symmetrical balance (Spemann, Brachet). In the second case, on the contrary, the first segmentation plane does not coincide with the plane of bilateral symmetry, but is at right angles with it. It cuts the egg into a dorsal blastomere which contains all the formative materials of the dorsal organs, and a ventral one

which contains only the materials for the future ventral part of the embryo. Here any alteration in the ventral blastomere cannot make it produce more than its germinal localizations allow. There are then in the egg, separated by a plane perpendicular to its plane of bilateral symmetry, a dorsal half containing the localized materials and factors necessary to produce a complete embryo and particularly the dorsal organs, and a ventral half that cannot produce any dorsal organs. Let us remark that it is about the same with the egg of *Rana fusca*, where the dorsal portion of the embryo, when it is taking shape, overruns the ventral half only at its caudal extremity (fig. 4).

Probably in *Triton*, as in the frog, between the two extreme positions of the first division plane with respect to the plane of symmetry: coincidence or perpendicularity, there may be a series of intermediate positions. But as in *Triton* we can only infer them from the results of separating the blastomeres and as these results are intermediate and often difficult to analyze, they are doubtlessly considered as experimental failures.

Summing up, we can say that the egg of *Triton*, either fecundated or segmentated into two, must have a localization of its developmental factors much like that of the frog's egg; but it cannot be directly examined, and that is certainly a disadvantage.

But this disadvantage has a compensation in one property of the egg which opens to the experimental analysis of germinal localization a field that cannot be studied well with the frog's egg; this property is a greater malleability of organogenetic substances, a less irreducible fixedness in the organogenetic properties of the various egg areas. An instance of this has been given already: when in *Triton* the segmentation plane coincides with the plane of first bilateral symmetry, if we separate the right blastomere from the left, each of them gives not a half embryo, but a smaller complete embryo, which is normal except for a certain asymmetry in the inner organs (Spemann) of which I cannot speak here.

The constitutive substances of the right and left blastomere may then easily change their place on either side of a new plane, parallel to the egg's plane of symmetry; they may slide, in a way, towards the right for one and towards the left for the other and reestablish in each blastomere a structure similar to that of the whole egg before segmentation. This alteration is accomplished, normally and easily, because, it seems, each blastomere is isolated and instead of touching its associate along a plane surface, can become spherical. The *Triton* egg shows itself then as a "regulation" egg, to use a well known expression. It is not the only known case; there are many others; for instance, that of *Amphioxus* (Wilson), and of the echinoderms; and for all the same explanation may be given. But there is more still: this power of alteration is present not only in the first two blastomeres; it persists until gastrulation, and the two halves of a blastula can each still produce an embryo.

An important fact to remember is this: that the autonomous alteration can only be brought about with the germinal localizations contained in the blastomeres; it can only establish the materials it contains according to the direction of the first segmentation plane and to the plane of symmetry and probably cannot create any new ones. As a proof of this we see that in *Triton*, if the first segmentation plane is perpendicular to the plane of bilateral symmetry of the egg, the first two blastomeres are not equivalent but always remain different; as has been al-

ready said, the ventral blastomere cannot create any axial organs, while the dorsal one produces a larva containing a complete nervous system, a dorsal chord, mesoblastic somites, etc.

This condition is very important and very likely it can be assumed for all eggs capable of regulation. The possibility of a part becoming equal to the whole emerges from the mystery that certain speculations had wrought around it. It allows us also to link the mosaic eggs, that is, eggs with a strictly determinative development, with eggs capable of regulation, that is, eggs that may undergo some alterations.

In the frog's egg, after destruction of a right or left blastomere, the surviving one always gives a half-embryo unless we turn it upside down; a certain degree of alteration becomes then possible. This shows that the differences between the frog's egg and the egg of *Triton* are not so fundamental as one would think at first sight. The technical impossibility of freeing the frog's egg from its vitelline membrane—an easy operation in *Triton*—does not allow the separation of the first blastomeres; if we kill one of them by a puncture, it remains in contact with the surviving one and may merely by its presence prevent alteration in the other. Nevertheless, McClendon's experiments, although incomplete, in which he could take out one of the two blastomeres while leaving the other in its place, would seem to show that even under much more favorable conditions the alteration does not take place. The question is then still open.

Other facts which we must examine, to complete this discussion, will show us how fruitful and useful for the progress of science is the difference in the behavior of germinal localizations, obtained by experiments, between urodelan and anuran eggs. They will force us to go more intimately into the analysis of the properties of germinal localizations and the real part they play. In the urodels (*Triton*) the very ingenious and well conducted experiments of Spemann, O. and H. Mangold, Ruud, Marx, and Geinitz have shown and explained a series of very interesting facts.

THE "ORGANIZER" AS A MORPHOGENETIC CENTER

Spemann's fundamental experiment, of which the others are merely variations, is as follows: in a triton's egg at the beginning of gastrulation, that is, at the moment when the cranial lip of the blastopore appears, we take off a median fragment of that lip or the immediately underlying part, seeing to it that in the cut off part there be not only elements of the future ectoblast but also of the presumptive endoblast (or ento-mesoblast); if then we transplant that fragment into the ventral part of another egg which is in the same stage or thereabouts, it easily embodies itself in its host and develops. But it not only undergoes its normal self differentiation, that is, producing that part of the embryo's dorsal organs the materials of which it contains; it also acts on the host's cells situated above the implantation spot, which would normally give only ventral organs (epiblast of the body's ventral part, mesoblast of the lateral plates, and digestive hypoblast) and it gradually excites in them, by a kind of induction towards the upper pole, the formation of a rough cast dorsal embryo with nervous system, chord and mesoblastic somites all well formed. The material of the blastoporal region of a young gastrula is, then, able to change the neighboring cellular groups from their normal development and to give them certain morphogenetic properties that

they did not possess, or that at least would have remained hidden and unknown without it. Spemann has given the name of "organizer" to that part of the cranial blastoporal region that can give such a formative impulse, and he considers that "organizer" as a real morphogenetic center which concentrates in itself all the factors that excite development in a triton's egg, the appearance and organogenesis of the dorsal organs characteristic of a vertebrate's larva—central nervous system, dorsal chord, mesoblastic somites, etc.

This conclusion finds also its justification in another experiment which is a kind of counter-proof of the first. It is well known in descriptive embryological research, and W. Vogt has recently confirmed it, that with the urodels and especially in *Triton*, these dorsal organs are topographically formed from the materials situated above the blastopore's cranial lip, between it and the egg's upper pole. We can, then, in the upper hemisphere of the young gastrula, draw the outline of the future medullary plate long before it appears: that is to say, there is a virtual medullary region long before it becomes real.

Now if from that region we cut off a small portion of ectoblast, above the region where the organizer is located, and if we transplant it into the ventral region of another gastrula, that fragment, which in its own place would have become a part of the central nervous system is embodied in its surroundings; it simply becomes ventral ectoblast and later on part of the epidermis lining the inner ventral surface of the body.

On the other hand, a fragment of the ventral ectoblast, which, remaining in its own place, would only become common epidermis, when it is transplanted in the presumptive medullary region of another gastrula is transformed in the new place it occupies into part of the central nervous system.

The conclusion we can infer from these experiments is, then, that in a very young gastrula, the cellular materials which will normally make the medullary plate, the forerunner of the whole central nervous system, do not do so because of a self determination, by self-differentiation; their destiny depends completely on the organizer which is beneath them in the blastoporal lip. This is a real proof that, after all, all developmental factors of dorsal organs are localized in it. These do not undergo a spontaneous but a stimulated development.

It is necessary to remark, though, that Goerttler has recently expressed doubts on the absolute indifference, in early stages, of presumptive medullary materials, in *Triton*. By means of a different method from Spemann's, that of punctures producing localized destruction, he has been able to observe a slight medullary differentiation of those presumptive elements even while depriving them of the organizer's influence. They would then be already in some way predetermined before they come to occupy their final location. This reservation made by Goerttler, if it is verified, has a far from negligible importance. But the importance of the organizer as the center of a differentiation from a distance would still be very great.

Finally, the analysis of the organizer has been continued in recent years by Geinitz, who has demonstrated the fact of its negative, or, at least, relative specificity. We knew already from the investigations of Spemann and H. Mangold that the organizer of a species of *Triton* could act on the germ of another nearly-related species. Geinitz has shown that these results can be extended to other urodels; the organizer of *Pleurodeles* or of

Axolotl implanted in the young gastrula of *Triton* produces in the host a secondary embryological rudiment. But what is most unexpected is that the organizer of an anuran, particularly of *Bombinator pachypus* acts exactly in the same way; it not only undergoes its own self-differentiation, but it produces by induction a development of dorsal organs in the triton material in which it is implanted. This observation is very interesting biologically, but it makes necessary certain remarks the importance of which will be shown later.

What Geinitz calls "organizer" in the Anura is the region situated just above the upper blastoporal region; and indeed this behaves like an organizer when it is implanted into the ventral region of a triton gastrula. But we shall see later on that the anuran organizer is localized long before the blastopore appears, and, at the stages when Geinitz operated, its action on the egg had been long felt, perhaps it was even at the end of its inductive power. This is probably the reason for Geinitz's observation, although, under very difficult experimental conditions, an urodelan organizer implanted into an anuran gastrula does not excite any development and only undergoes its own self differentiation. In fact this stage is in the Anura too much advanced; the evolutive determinism of the different regions is already fixed and can no longer be changed.

It is clear, nevertheless, that there is in this field, matter for new research work that may give very interesting results.

Two more points must be noted to complete the analysis of the investigations of Spemann and his collaborators. One is a quite obvious fact: in the organizer the essential part, from the standpoint of its morphogenetic action, is the entoblastic region; the ectoblast is by itself ineffective. Marx and Geinitz have recently given some new proofs of the part played by the dorsal endoblast (endo-mesoblast) in the differentiation of the medullary plate at the expense of the ectoblast that covers it: these proofs confirm the earlier observations of Spemann and Mangold.

The other point, established and confirmed by various experimental combinations of Spemann's school, is of a chronological kind. All the experiments on *Triton* here described only give complete results when the egg has arrived at the stage at which the researches were made: the young gastrula with a barely outlined blastoporal lip. A little later, when the blastopore is completed and has shrunk into a long slit, the developmental determination of the germ's various parts is fixed. The ventral ectoderm no longer reacts in the presence of an organizer; it continues its normal development; a presumptive medullary plate fragment implanted in the ventral part is no longer altered under its influence and produces what it would have produced if it had been left in its place. The advanced gastrula has become a mosaic of potentialities that can no longer be altered. This fixation could naturally be foreseen; it was nevertheless interesting to determine, by experiments, the exact moment when it occurs.

But there is another question not yet answered. Is the organizer, which occupies the region of the cranial blastoporal lip at the beginning of gastrulation, localized in that exact spot since the beginning of development, and was it already in that same spot with all its potentialities in the fecundated egg? Or is it the result of a concentration, in that same spot, during the stages that immediately precede the gastrular invagination, of materials scattered in a region that is larger in the transverse direction, near the equatorial region of the egg?

Goerttler's observations by means of Vogt's method of vital staining, and others described by Vogt himself, seem to show the second alternative as more likely. These authors have noted, in fact, when gastrulation is beginning, some cellular movements that seem equivalent to a real establishment of primordial rudiments, and it is then quite likely that the stage which Spemann studied is preceded by others where morphogenetic factors are differently scattered over a larger region.

It ensues from this that the final establishment of those factors, characterized by the formation of the organizer of the dorsal organs, is in the urodels a slow process that ends rather late. Already, in the first pages of this paper, have we remarked that, as a result of Spek's latest researches, the same localization in the ctenophore egg is established much earlier, although it is only ended in the 8-blastomere stage. We have insisted on the fact that in the Anura it is earlier still, since it seems to be established and finally fixed with all its characteristics as soon as fecundation is realized, and is made obvious by the outward appearance of the gray crescent. This explains many differences between the results of experimental interventions with the Urodela and the Anura, and it allows us in the latter, to discover new and more general properties of germ localizations, that are seen with great difficulty in the urodelan egg.

In the Anura and especially in *Rana fusca*, of which I shall particularly speak, the normal development of the embryo is a little different from what it is in *Triton* and in a general way in the urodels. In these, where the appearance of a prominent blastoporal lip is the first topographical mark allowing a localization of the rudiments, the greater part of the embryo's dorsal region is formed above it, towards the upper hemisphere. This has been shown by the researches in descriptive embryology, and the experimental work of Spemann has given what seem to be final proofs. In the frog, where an early exact mark is furnished by the gray crescent, numerous descriptive and experimental studies, that we have already briefly described and that are fully confirmed by recent results of which I shall speak later, have shown that the gray crescent contains the rudiment of the region that, at the moment of gastrulation, the upper (cranial) lip and the neighboring parts of the blastopore's lateral lip will occupy.

Later on these lateral lips will spread and reach the lower pole, near which they unite into a lower caudal lip. But while it spreads in this way, the blastopore becomes closed by a concentration of its cranial and lateral lips; as the end of this concrescence the blastopore is reduced to a small hole in the caudal extremity (fig. 4) all along that closing line, and at the expense of the cellular materials of the blastoporal lips (that is, of the gray crescent, and of the regions that are its prolongation towards the lower pole) an ecto-endoblastic plate is formed which produces the dorsal surface of the primary intestine and in which will be formed the rudiments of the central nervous system, the chord, the epithelium of the digestive tube and the mesoblastic somites. We can then say that the body of the *Rana* embryo is of blastoporal origin; only the prechordal fore-head, that is to say, the region of the fore-brain characterized by the optic and olfactive regions, is formed out of the cranial lip, towards the upper pole. It is the only part of the body that is formed, in the frog, according to the urodelan manner: from below to above; all the rest is made in the other direction, from above to below. Notwithstanding

the reservations made recently by Vogt on the absolute exactness of these facts, we can consider them as the more solidly established because they exactly correspond to the results of recently published experiments (Brachet, 1923).

RESULTS OF DESTROYING CELLS OF THE GRAY CRESCENT

In certain frogs' eggs the gray crescent is perfectly visible, and although segmentation hides a part of it, it still remains visible in the blastula, a few hours before the blastoporal lip begins to rise. We can then destroy, with a small slightly heated needle, a part of the blastular region covered by the gray crescent, either in the middle, or in one of its lateral horns. The group of cells destroyed by the needle or the heat, is for the greatest part ejected from the egg after a few hours and a scar is formed. A few dead cells remain in their place, and allow us sometimes, even 48 hours after the operation, to recognize in the embryo the injured spot.

The aim is to prevent or alter the development of the potentialities of the cells in the gray crescent; and we can see at once that these kinds of experiments are in fact the reverse of those Spemann and his collaborators have applied to *Triton* in more advanced stages.

The results of these experiments, varying in proportion with the intensity and localization of the puncture, but remarkably linked together, allow us to draw the following general conclusions. The median part of the gray crescent, which is also the largest (fig. 1) and which is cut in two by the blastula's bilateral plane of symmetry—which is also the egg's plane of symmetry—and where the blastopore's cranial lip will later arise, is the only part liable to undergo an autonomous differentiation, that is to say, that its development seems only due to the forces it has in itself; it only gives the fore part of the head's chordal region. But the activity developed in that important spot is really the initium of all the embryo's dorsal morphogenesis; it stimulates progressively, by a real induction, the differentiation of the materials situated above it, for the formation of the prechordal head, and to a greater extent, the differentiation of the elements of the crescent's lateral portions as well as their prolongation towards the lower pole, which will form the nape of the neck, the trunk and later on the tail.

This nodal point formed by the middle part of the gray crescent gives then the condition of progress for the potentialities held in the gray crescent's lateral horns, and in a more general way, of all the blastula's materials which will help to form the embryo's dorsal organs. It behaves, as Spemann says, as an organizer; it has from the morphogenetic standpoint the same value as the region surrounding the upper lip of the triton blastopore. This organizer of the frog's blastula is nothing else than the middle part of the gray crescent of the fecundated egg; for this reason we cannot doubt that if the blastula's localized destruction could be realized on the fecundated egg, the same results would ensue.

The experiments here related show another important point. Under the conditions in which they have been performed, we can clearly see that the organizer of the frog's egg can only induce already predestined materials to develop; it has no influence on other materials. This is another difference between the frog's egg and that of *Triton* and it explains Goerttler's failures in his transplantations of the triton organizer into frog blastulas. It is possible, though, that if Goerttler had tried implantations

on earlier stages he might have obtained very different results. Such verification experiments are, however, very difficult to perform.

There is another kind of fact which gives us some idea regarding the remarkable properties of the germinal localizations in the frog's egg. If, for instance, we prick with a heated needle the gray crescent substance a little to the right or left of its middle part, and if the injury is sufficient to destroy all the active material —or material activated by induction— no organ is formed either at the level of the injured spot or at the back of it. Nevertheless the blastopore's lips arise on either side; but while on the healthy side the lip material differentiates into a nervous half-system, a half-chord, a row of somites, all well-made and normal, on the injured side no organ is formed and in all the blastoporal lips down to the rudimental of the caudal bud there is only a cellular proliferation. This kind of embryo never lives longer than three days after the operation.

If the destruction of the gray crescent material is only partial, the formative induction can progress. But it is weaker and we can still see on the injured side a central nervous half-system, a half-chord, and a row of somites, but they are much smaller than the normal ones; they are like miniatures of normal organs. They may, nevertheless, be perfectly well made and undergo their normal histogenesis (fig. 7) if they can acquire a sufficient bulk.

Evidently, if we perform a great number of experiments, we can obtain a great number of quantitative variations between the total suppression of morphogenesis and a complete and normal morphogenesis, we can obtain a whole series varying in degree. But the conclusion we draw from them all is that the bulk of the organs differentiated by induction is in proportion to the bulk of the cellular body.

This quantitative idea becomes even more interesting if we state that the deficiency produced in this way in the dimensions of the organs is indelible and can never be won back. We can, in fact, keep alive, for several days after the operation, larvae that have undergone a unilateral (right or left) lesion if not too serious and which have on that side a nervous half-system greatly reduced in bulk but not in length, a series of somites and eventually even a dorsal chord. At that moment these larvae are small tadpoles, freely living in water, provided with gills, a normal heart, and freely circulating blood. The two halves of the body are identical; they are normal in length, and have both undergone their functional histogenesis, but the reduced side remains smaller and does not win back anything it lacks, although the conditions of recuperation if it were possible, are well provided.

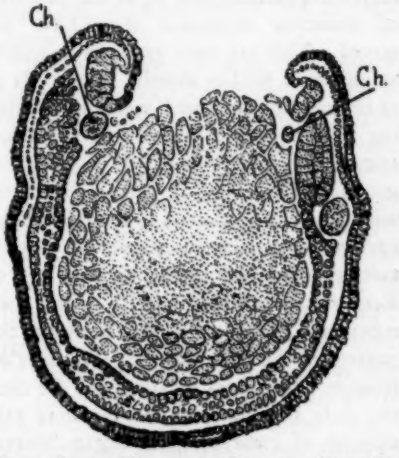

FIG. 7. SECTION THROUGH AN EMBRYO OF *Rana fusca* DEVELOPED AFTER THE PARTIAL DESTRUCTION OF THE RIGHT HORN OF THE GRAY CRESCENT IN THE BLASTULA STAGE

On that side the nervous system and dorsal Chord (*Ch.*) are much smaller than on the uninjured side.

LIMITS TO GROWTH

This statement brings up the question of the growth of the organism and of its limits and consequently the reasons for the limitation in the length of the organism. The fact that organs formed with too little initial substance remain small and can never win back what they lack, shows that the materials they are made of, have a limited growing power because of their composition, that is, of the quantity of inductive substance they have received. This can only grow by nutritive assimilation within definite limits. It is as though the substance wears away during morphogenesis without being reconstituted by assimilation. As the organogenetic regions already exist in the frog with all their properties in the fecundated egg, we may infer this simple but important formula, that the fecundated egg of *Rana fusca* contains in itself, in its own substance, the conditions limiting the growth of the organism that will develop from it. Germinal localizations are then not only the material and dynamic substratum of embryological organ-differentiation, but rule also the size of the adult animal.

The reader will see from all this that the researches on anuran and urodelan amphibians do not contradict one another as some people think, but on the contrary, they complement one another. Each of the two groups furnishes good material for study, exhibiting very luckily for scientific progress, similar phenomena in different ways: in the anuran egg germinal localizations occur very early and are quickly fixed and established; the possibilities of alterations are, for them, very early inhibited, perhaps simply owing to the too great viscosity of the cytoplasm. In the urodels, these same processes being much slower, are only finished when gastrulation is in an advanced stage; alterations in germinal localizations are possible until then and the determination of the germinal regions is only then fixed; this probably is due to a lesser viscosity in cytoplasm. In all the species studied up to the present time germinal localizations, with some variations, may be compared to the anuran or urodelan type or else may be intermediate between the two.

PROBLEMS OF THE NATURE OF GERMINAL LOCALIZATIONS

In this essay the actual state of germinal localizations has been set forth as it appears to the author according to the most recent investigations. We have refrained, as a rule, from any speculative, theoretical view, and have kept to facts of experiment and observation and the conclusions we could justly arrive at from them; we have been able to link them in a coherent whole which is at least good in this sense, that it gathers together problems not yet solved and clearly organizes data regarding them. Although the work already done is, from this standpoint, encouraging, and even satisfactory, it furnishes only the first steps on a road that remains broadly open to science. We know that developmental factors exist and that they are localized in the germ; we sometimes obtain a view of the establishment of these germinal localizations; we know some of their properties, but we know nothing as yet of their intimate nature. The most important study, still to be undertaken in science, is precisely the investigation of the nature of the germinal localizations and for the elements of the germ from which they are derived. Germinal localizations, which are specialized zones in the egg, have certainly a peculiar chemical composition and physical state; each

formative zone must have its own formula that differs from the others quantitatively and qualitatively, and it should be possible to make a graphic outline of the variations it undergoes during all its evolution. The superposition or combination of these studies, when they are made, will be the physico-chemical expression of embryological development. This analysis will require much time and work, but it must be done to help the progress of science. Without waiting for that result, embryologists who are interested in the biological aspect of their science,—and they are more and more numerous—have still an important program of analytical research.

The germinal localizations here discussed are the fundamental localizations that can be traced back to the egg. They are those that contain the materials and energies destined to produce, not one organ or one system of organs, but a whole region of the body. We may recall in fact, that the most exact data have been acquired by the study of the formative centers of the dorsal organs of amphibians. Factors that are localized and recognized are really factor complexes and we ought to dissociate them into their elements, and then see the reciprocal actions among them and between them and the rest of the germ. From this standpoint Hoadley has recently published some very interesting researches on the blastoderm of birds. The primordium from which the eye will come, for instance, has a different development if it is isolated and transplanted in the allantoic chorion of another chicken's embryo; the development depends on the stage of the germ's development at the moment of isolation. Its potentialities are not all simultaneously established; they come progressively in a certain order; at the beginning this primordium can only produce a vesicle whose cells are colored by a pigment; a little later it can produce a rudiment of a secondary vesicle, that is to say, invaginate with the rudiment of a visual sheet, and so on. Hoadley has made similar observations for other organs also, and particularly for the excretory apparatus.

We do not know how far these observations may be used for generalizations, but the notion that each germinal localization, creating an organ system, still undergoes an inward development after it has appeared, is worth notice. It is likely, though, that observations will show that this movement may be rapid or slow, depending on the species; in some the localization will appear together with all its properties, in others we shall be able to catch by experiment the various stages of their succession; this is true for the final establishment of the great germinal localizations in the egg of the Anura and Urodela.

Besides the interesting problem of developmental factors, which is the only problem discussed in this paper, there are a great number of recent investigations on the development of the branchial apparatus, of the heart and especially the limbs, to which Harrison and others after him have devoted a series of remarkable investigations. But these are outside of our province.

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THE VISCOSITY OF PROTOPLASM

By L. V. HEILBRUNN

University of Michigan

GRAHAM, the founder of colloid chemistry, called the viscosimeter a colloidoscope. He felt that the problems involved in the study of colloids were most intimately bound up with the changes in their viscosity.

At the present time the physical chemist can study the properties of colloids in a number of ways. But for the investigator of the colloid chemistry of protoplasm the measurement of viscosity is at present almost the only means of approach.

The physical and chemical study of protoplasm is never easy. The intrinsic difficulties are obvious. The colloid chemist has large masses of material at his disposal, but the biophysicist must be content with droplets too small for ordinary vision. Moreover in the manipulation of these microscopic droplets he must be extremely cautious. Almost anything he does is likely to cause death, and once this occurs the protoplasm is no longer protoplasm.

Small wonder then that for so many years students of the living cell were content to do their physical thinking in terms of loose, general speculation. It is much simpler to argue about the physical properties of protoplasm than actually to measure them. But the physical study of protoplasm is by no means impossible. The biologist may never be able to make measurements with the precision which the physicist is able to employ in his study of easily accessible inanimate objects. On the other hand measurements can be made and they are being made.

METHODS OF MEASURING VISCOSITY

Perhaps the pioneer in the viscosity measurement of protoplasm is the German botanist Heilbronn. Before him various biologists had observed the flow of protoplasm and had used rough descriptive terms in discussing its viscosity. Reinke (1895) and Albrecht (1898) noticed that the flow of protoplasm from crushed sea-urchin eggs was slower after fertilization. Workers using the centrifuge in studies of the organization of the egg and the potentialities of its parts, commented now and then on the fact that granules moved through the egg more readily at certain times than at others. Indeed Fauré-Fremiet (1913) emphasized the important point that there were differences in viscosity in the protoplasm of the eggs of *Ascaris* at different temperatures. But it rather seems as though Heilbronn was the first to systematically attempt real measurements of the viscosity of living cells. He observed the speed with which starch grains fell through the protoplasm of certain plant cells. His first studies were on the cells of the leguminous plant *Vicia faba* (1912, 1914). From a comparison of the speed of fall of the starch grains in protoplasm and in water, Heilbronn was able to determine the protoplasmic viscosity. The method that Heilbronn used depends

on well known physical principles and is in fact a standard type of viscosity measurement.

At about the same time that Heilbronn was beginning to measure the protoplasmic viscosity of plant cells, another type of viscosity determination was being undertaken for animal cells. Kite (1913) very cleverly applied the Barber pipette method to the microdissection of cells. Using extremely fine needles, he cut into a wide variety of cell types under various conditions and by watching the progress of the invading needles he attempted to make estimates of the viscosity. Kite's estimates were never in any sense measurements. He described certain cells and parts of cells as gels, others were sols, and some of the cellular gels he thought of as being stiffer than others. The work of Kite was later taken up by Chambers (1915, 1917a and b, 1919) and then by Seifriz (1918, 1920, 1921). The latter tried to introduce a scale of viscosity values by comparing the passage of a needle through protoplasm and through various chemical substances. This was really an attempt to make microdissection a quantitative measure of viscosity. The possibility of the success of such a procedure is rather doubtful. That anyone should be able to measure viscosity accurately by observing the passage of a mechanically controlled needle through a fluid seems on the face of it unlikely. It would be very easy to put the matter to a test. If a microdissectionist were given certain unknown samples of fluids, it would be interesting to see how his estimates of their viscosity compared with the known values.

As a matter of fact the estimates of viscosity furnished by most microdissectionists are probably based largely on their observations of the speed of flow of protoplasm from punctured cells. If this is the case the method is not fundamentally different from the older type of work in which biologists studied the escape of protoplasm from crushed or compressed cells and estimated its viscosity by receiving a mental impression of the speed of flow. Used in this way the microdissection method may serve as an indicator of large changes of viscosity, provided that the reaction which occurs at the surface of the emerging droplet of protoplasm is the same during the progress of the experiment. As protoplasm emerges from a cell it immediately becomes invested with a film. In a paper now in press (1927) I have called the reaction which occurs at the surface of emerging protoplasm the surface precipitation reaction. I have found, moreover, that the reaction varies widely under various external and internal conditions. A thorough understanding of this reaction may contribute to a greater precision of the microdissection method. Until such an understanding is reached we can never be certain of which factor is involved in the speed of escape of protoplasm from a punctured cell. If for example the protoplasm flows out very slowly, this may be due either to a lowered viscosity or it may be due to an increase in the speed of the surface precipitation reaction.

The gravity method of Heilbronn, although very useful for certain types of plant cells, can not be employed for the determination of the protoplasmic viscosity of animal cells. For these cells a force greater than gravity must be used to move the granules through the protoplasm. Centrifugal force serves the purpose very nicely. With the aid of a centrifuge it is often a very simple matter to throw the granules of a cell to one pole. The speed of the granules can then be determined with a moderate degree of accuracy, and in this way it is possible to obtain a meas-

ure of the viscosity. The technique of viscosity measurement by the centrifuge method has recently been described in detail (Heilbrunn, 1926a).

In addition to the gravity and centrifuge methods of measuring protoplasmic viscosity, several other types of measurement have been employed. Heilbronn (1922) devised an ingenious magnetic method. He introduced iron particles into the protoplasm of slime molds and measured the speed with which they were attracted to an electromagnet. This method, as Heilbronn himself points out, is probably not generally applicable, for large metal particles could scarcely be introduced into other types of cells without injury.

Several authors have observed the Brownian movement in protoplasm and have estimated the viscosity from the rapidity of such movement. Ordinarily this has been done by watching the protoplasm under darkfield illumination (Leblond, 1919; Bayliss, 1920). Such observation can furnish evidence as to very pronounced changes in the viscosity of the material in which the protoplasmic granules are suspended.

It is also possible to determine the speed of Brownian movement in another way. When cells are centrifuged the granules thrown to one end of the cell slowly migrate back to their original position. The speed of such a return migration depends in part at least on the speed of the Brownian movement. By measuring the rate at which the granules return one can obtain a measure of the protoplasmic viscosity. Unfortunately, however, the linear distance traversed by a particle in a given time varies inversely not as the viscosity itself but as the square of the viscosity (see Burton, 1921). The method is therefore not a very delicate one, although it does have certain advantages. A few preliminary measurements have been made with it.

Finally mention should be made of the plasmolytic method of estimating protoplasmic viscosity. This has been developed by various botanists (Weber, 1921a, 1924a, 1925a, b, c, and d; Cholodny, 1924; Scarth, 1924a). When a plant cell is placed in a hypertonic solution it usually shrinks from the cell wall in a round or oval mass with a smooth contour. This Weber designates as *convex* plasmolysis, and it is an indication of a relatively low viscosity of the interior protoplasm. Following an increase in the protoplasmic viscosity, the plasmolysed cell may show instead of a smooth contour, an uneven rough border ("eckige Plasmolyse"), or it may retain attachment to the cell wall by strands of protoplasm ("Krampf-Plasmolyse"). Such formation of strands is in part due to an increase in the adhesive properties of the surface layer of the cell. In *Spirogyra*, an increase in the rigidity of the chromatophore may cause a type of plasmolysis which is called "Schrauben-Plasmolyse." That these different types of plasmolysis are really associated with changes in the viscosity of the interior protoplasm has been demonstrated by Weber. By centrifuge tests he has shown that when convex plasmolysis occurs, the viscosity of the protoplasm is relatively low. Following coagulative changes in the interior of the cell the plasmolysis changes from the smooth convex type to the uneven or "eckig" type.

Further discussion of the methods of viscosity measurement for living cells is scarcely necessary. An excellent review of the entire subject has recently been published by Weber (1924b), and the reader is referred to this source for further details.

It should be pointed out, however, that the various types of viscosity determination are not always concerned with the

same thing. Thus if one estimates the viscosity from the speed of Brownian movement of a granule, the viscosity value arrived at is not so much that of the entire protoplasm but rather that of the medium between the granules. On the other hand the gravity method, the magnetic method, and the plasmolytic method should give values which represent the viscosity of the entire protoplasm, granules and all. Probably this is also true for the centrifuge method (see Heilbrunn, 1926b), although this method may give a value intermediate between that of the entire protoplasm and that of the fluid between the granules.

VISCOSITY OR PLASTICITY

Perhaps it is not quite proper to speak of the viscosity of protoplasm. It may be that protoplasm generally is a plastic solid rather than a viscous fluid. If this were the case it would be more correct according to some authors, (see for example Bingham, 1922), to speak of its plasticity rather than its fluidity or viscosity.

Many materials that we ordinarily regard as fluids are really plastic solids. Paint, for example. When subjected to only a slight force, plastic solids do not flow at all. The force acting on them must first overcome a certain resistance, and then the flow may be quite rapid. For this reason in plastic solids the rate of flow is not exactly proportional to the impelling force or the shearing force. On the other hand in truly fluid materials we generally find an exact proportionality between the shearing force and the rate of flow.

So far only one set of observations has been made which might serve as a test to decide whether protoplasm was a true fluid or a plastic solid. In a recent study of the protoplasm of marine eggs (Heilbrunn, 1926a), it has been found that for a certain range of values at least, the rate at which the granules move through the protoplasm is proportional to the shearing force. Hence in this type of protoplasm it is probably true that we are dealing with a fluid rather than with a plastic solid. But it is possible, and indeed even probable, that many other types of protoplasm are plastic solids.

THE ABSOLUTE VISCOSITY OF PROTOPLASM

Most students of the viscosity of protoplasm have been content with comparative values. The viscosity at one time, or under a given set of conditions, is found to be greater or less than at another time or under another set of conditions. Only in a few instances has the absolute viscosity of protoplasm been measured. Table 1 shows these measurements. The viscosity measurements are given in absolute units. Water has an absolute viscosity of approximately 0.01. It is necessary therefore to multiply the figures in the table by 100 in order to obtain the viscosity in terms of that of water.

Not all of the above measurements are equally trustworthy. Seifriz in using the magnetic method introduced nickel balls 16 microns in diameter into egg cells only 150 microns in diameter. Such large foreign objects would almost certainly produce injury.

The measurements show a wide divergence in the viscosity of protoplasm. The material of the sea-urchin and *Cumingia* eggs is relatively fluid. The granules of these eggs are suspended in a liquid with the viscosity of a typical protein solution. The presence of the granules themselves adds to the viscosity of the entire mass. Various authors have suggested formulæ for the calculation of the viscosity of a suspension, if the viscosity of the suspen-

sion medium and the concentration of suspended material is known. Another formula, that of Bingham (1922), requires a knowledge of that concentration of the suspension at which the viscosity becomes infinite. Inasmuch as the concentration of the protoplasmic suspension can be increased by decreasing the volume of the cells, it is possible to determine the concentration of granules at which the protoplasmic viscosity becomes infinite. It is only necessary to shrink the eggs in hypertonic solutions, and by centrifuge tests to determine the exact point at which the viscosity becomes infinite or approaches infinity. Application of the formula of Bingham as well as the formulæ of Einstein and Hatschek shows that the viscosity of the entire mass of the protoplasm of sea-urchin and *Cumingia* eggs is about two or three times that of the fluid between the granules, (see Heilbrunn, 1926c). The marine egg protoplasm and that of the plant cells studied shows a relatively low viscosity. On the other hand the viscosity of *Paramecium* protoplasm is unusually high. In this determination Miss Fetter centrifuged large food vacuoles of known specific gravity and measured their speed of movement through the protoplasm. In such a study the movement of the vacuoles would be greatly retarded by any formed elements in the protoplasm. With our increasing knowledge of the complexity of various Protozoa it is not at all unlikely that such formed elements exist within the *Paramecium* cell and serve to resist the passage of the vacuoles. Rees in his figures of the neuromotor apparatus of *Paramecium* (1922) apparently shows it ramifying into the endoplasm, in which position it would doubtless tend to prevent a passage of the vacuoles of the cell. It would be interesting to determine whether the protoplasm of *Paramecium* behaved as a plastic solid. This could easily be done by centrifuging at different speeds (see above).

TABLE 1

Absolute viscosity measurements

MATERIAL	METHOD	ABSOLUTE VISCOSITY	AUTHORITY
Starch sheath cells of *Vicia faba*, a leguminous plant	Gravity	0.08–∞ Average value 0.24	Heilbronn, 1914
Various species of slime molds just before fruiting	Magnetic	0.09–0.185	Heilbronn, 1922
Eggs of sand dollar, *Echinarachnius*	Magnetic	Slightly less than 8.0	Seifriz, 1924
Sea-urchin eggs, granule free protoplasm	Centrifuge	0.02	Heilbrunn, 1925
Sea-urchin eggs, entire protoplasm	Calculated from previous value	0.04–0.06	Heilbrunn, 1925
Cumingia eggs, granule free protoplasm	Centrifuge	0.04	Heilbrunn, 1925
Cumingia eggs, entire protoplasm	Calculated from previous value	0.12	Heilbrunn, 1925
Paramecium	Centrifuge	80	Fetter, 1925

VISCOSITY CHANGES DURING PROTOPLASMIC ACTIVITY

Life is a succession and a medley of chemical changes. The energy derived from these chemical transformations is what makes vital activity possible. In

recent years the biochemist and the physiologist have had some success in tracing out a few of the many chemical reactions that occur in living matter. These chemical reactions must be related in one way or another to the physical behavior of the protoplasm. Most of the obvious manifestations of life are physical. Movement, the passage of an impulse along a nerve fiber, the division of a cell—all these involve physical mechanism. No amount of knowledge concerning the chemical transformations of protoplasm can in itself solve the riddle of what actually makes a muscle contract or a cell divide. The problem is essentially a physical one.

Living substance is colloidal—it is universally so. The activity of living things is almost certainly bound up with some sort of colloidal change. But what?

One peculiarity of colloids is the fact that they readily undergo changes in their physical state. Fluid one moment, in an instant they may be converted into a more or less rigid mass. Or vice-versa, a semi-solid gel can quickly become transformed into a fluid sol.

Physiologists generally, even before colloid chemistry emerged as a science, have always been ready to assume colloid chemical explanations of vital activities. Thus from the first, most theories of muscular contraction have postulated some sort of colloidal change. And colloidal explanations have been advanced for many other types of biological phenomena. The attempts to prove or disprove these hypotheses have in most cases not been very fortunate. Time and again physiologists have compared protoplasm to this or that inanimate colloid, and as a rule they took these comparisons much too seriously. Often enough biologists have made observations on gelatine in the hope that their results might throw some light on the mysterious behavior of living protoplasm. It is doubtful if there is any colloid more unlike protoplasm in its physical behavior than gelatine. This should have been obvious from the first, for the most striking fact about the behavior of gelatine is that it becomes more fluid as the temperature is raised, whereas protoplasm quite certainly undergoes the reverse sort of change. And this is only one of a great number of essential differences.

The only way to study the colloidal changes of living substance is to observe living substance itself. When the chemical composition and arrangement of living protoplasm become better known than they are at present, it may be possible to find non-living materials which to some extent resemble it in their physical behavior. Even now such studies might be pursued with profit. Most types of protoplasm are concentrated suspensions of visible granules. The colloid chemist has studied such concentrated suspensions very little, in spite of the fact that the small amount of work already done has indicated that the subject is one of great interest. If comparisons between protoplasm and non-living matter are to be made at all, it would doubtless be much wiser to compare protoplasm with types of material which in some degree resemble it. But after all, until the day comes, if it ever does, when we can actually synthesize living protoplasm, no inanimate material can be expected to show exactly the same colloidal behavior as protoplasm. If we are to really be sure of the colloidal changes of protoplasm we must study them directly.

In making observations on living cells, the most reliable index of colloidal change is the viscosity. In colloidal systems, when a sol changes to a gel, it assumes form and is as a result elastic; at the same time its viscosity undergoes a decided increase. No one has as yet made a proper study of the elasticity of cell protoplasm,

but as has been pointed out, the viscosity can be measured in a variety of ways. As a matter of fact the colloid chemist has often used viscosity change as an index of gelation. Thus to quote a leading authority, "The viscosity is the property which changes most strikingly during the sol-gel transformation, and hence has been investigated most closely" (Freundlich, 1926).

In a way it is rather difficult to compare observations on non-living and living systems. The terminology of colloid chemistry has been built around a study of colloids in test tubes. In a cell, conditions are different. Thus although it would probably be correct to say that any pronounced increase in the viscosity of protoplasm indicated that the protoplasm was a gel, it should be noted that increases in the viscosity of protoplasm might really be due to a variety of causes. In a test tube precipitation phenomena lead to a settling out of the precipitate with a consequent decrease in the viscosity of the remainder of liquid. But when substances are precipitated out of the protoplasm of the cell, they remain within the extremely narrow confines of the cell itself, and the viscosity of the entire protoplasm is increased. Changes in the viscosity of protoplasm might also be due to changes in the surface properties of the granules suspended within the cell, as well as to changes in the material between the granules. For a more complete discussion of this point see Heilbrunn (1926b). In the future it will not be enough to discover that protoplasmic viscosity increases and that the protoplasm becomes a gel, the question will be asked as to the nature of the changes that brought about the increase in viscosity.

Viscosity change during vital activity has been studied most in marine eggs. In a paper published in 1915 (Heilbrunn, 1915), it was shown that fertilization of the sea-urchin egg is accompanied by a sharp increase in viscosity and this observation has been generally accepted. Centrifuge tests show the difference in viscosity very markedly. With the same amount of centrifugal treatment unfertilized eggs show far more granular movement than do eggs fertilized fifteen or twenty minutes previously (at about 20°C.). Fatty granules or oil droplets move to one pole of the egg, colorless granules and pigment granules move to the opposite pole of the unfertilized eggs. In the eggs centrifuged for the same length of time and at the same speed twenty minutes after fertilization, there is no visible movement of granules at all. The difference is striking.

If sea-urchin eggs are centrifuged two or three minutes after insemination, the colorless granules of the egg move just as rapidly as before fertilization. On the other hand, not all the pigment granules are moved by the centrifugal force. In the cortex of the fertilized egg there is a larger number of pigment granules which have resisted the centrifugal force, than there are in similarly treated unfertilized eggs. This observation may be due either to the fact that at fertilization the pigment granules move out into the cortex of the egg where they become attached to the outer membrane and resist the centrifugal force or it may be due to the fact that after fertilization there is a cortical gelation or coagulation. Miss Hyman's experiments favor the latter interpretation (1923).

In a preliminary note published in 1917, and in the complete paper published in 1920 (Heilbrunn, 1917, 1920a), the viscosity changes during the cell division of the sea-urchin egg were described in detail. These results were later made more exact for the moment or two before cleavage (Heilbrunn, 1921).

At the very same time that this work

was being done and in the same laboratory, Chambers was making estimates of viscosity changes during cell division in the sea-urchin egg, using the microdissection method (Chambers, 1917b, 1919). Although at first sight his results might be thought to agree with those arrived at by the centrifuge method, they really differ in one or two important points. Some time later Seifriz (1920) also published a few estimates of protoplasmic viscosity during cell division.

From the published descriptions of Chambers and Seifriz it is not easy to picture the changes they believe to occur. This is in part due to the fact that Chambers frequently refers to various stages in terms of minutes following fertilization or minutes before cleavage, although obviously the number of minutes which elapse before a certain stage is reached is a function of the temperature. For the sake of clearness therefore it was thought worth while to plot Chambers' results, even though only the roughest sort of a curve could be obtained. In order to be certain to cite the facts as Chambers describes them, a condensed account in the words of the observer is here given. Quotations are from the 1919 paper.

> Immediately after fertilization the cytoplasmic granules readily flow by the moving needle. (This is taken to indicate low viscosity.) . . . . When the sperm aster is at its full development the highly viscous state of the cytoplasm is detected by the needle. . . . . This condition is at its height 10 to 15 minutes after fertilization. Fifteen to 20 minutes after fertilization, the radiations of the aster begin to fade from view, with a reversal in the cytoplasm of the semi-fluid to a more fluid state. . . . . Toward the end of this stage, which lasts for about 20 to 30 minutes. . . . . Shortly before cleavage, about 40 to 50 minutes after fertilization, an increase in firmness sets in. . . . . The time of appearance of the amphiaster until completion of cleavage lasts from 10 to 15 minutes. The increased viscosity of the egg during this amphiaster stage. . . . . After the completion of the cleavage process, there are indications that the firmness of the cytoplasm persists.

The account given in the 1917 paper does not agree absolutely with this account. After the amphiaster has formed, and shortly before cleavage, Chambers in 1917 is led to conclude from granular movements near the equator of the nuclear body, that a liquefaction occurs, at least in this region of the cell. But he says, "I was not able to convince myself on this point by the use of the needle."

Figure 1 shows the results obtained by the centrifuge method and those of Chambers on the viscosity of the sea-urchin egg during the period between fertilization and cleavage. The centrifuge data are for the most part those published in 1920, and are based on a number of observations. The viscosity changes shown by the centrifuge are represented by an unbroken line. Chambers' data are approximated by the line of dashes. Both curves show an increase in viscosity followed by a decrease. The outstanding difference is that Chambers regards the period during which the amphiaster is fully formed as a period of increased viscosity, whereas centrifuge tests show it to be a stage of low viscosity. Seifriz's microdissection estimates agree with the centrifuge tests in regarding the amphiaster stage as one of lowered viscosity. Thus he states (1920),

> With the first appearance of the amphiasters there is a pronounced decrease in viscosity of the central region of the cell, and this condition is maintained throughout the intermediate stages of division (from middle prophase to late anaphase).

In figure 1, the total time between insemination and cleavage is taken as fifty minutes. When the actual time was greater or less than fifty minutes a correction was made for all points plotted.

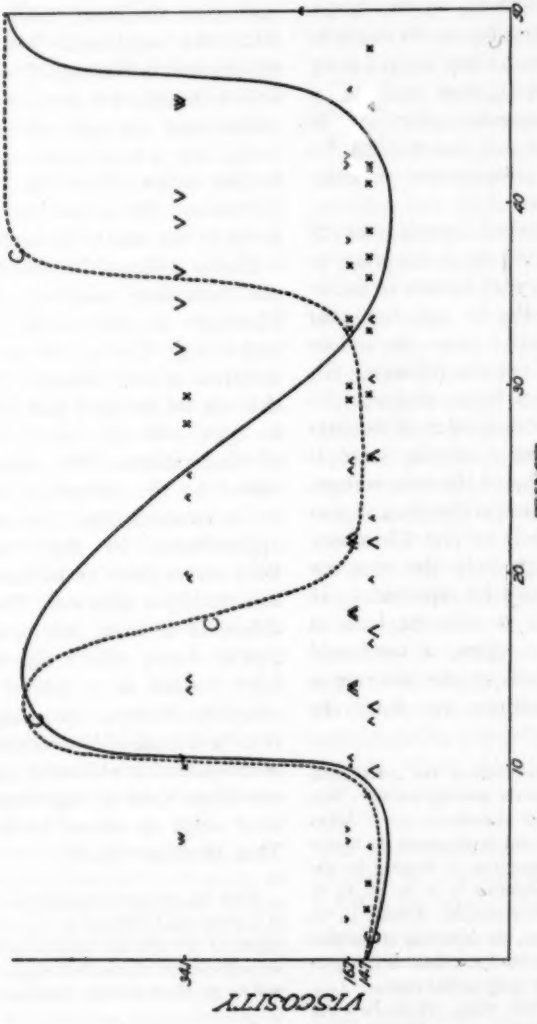

FIG. 1. VISCOSITY CHANGES DURING MITOSIS IN THE SEA-URCHIN EGG. THE UNBROKEN LINE IS PLOTTED FROM THE CENTRIFUGE DATA (HEILBRUNN), THE DOTTED LINE IS A ROUGH ESTIMATE BASED ON THE MICRODISSECTION EXPERIMENTS OF CHAMBERS

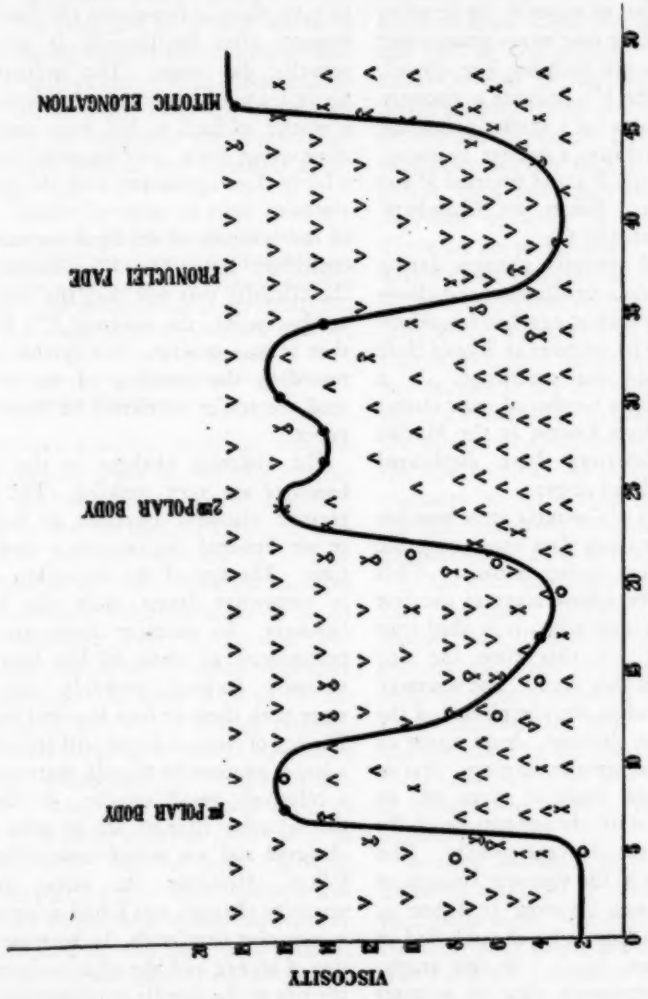

FIG. 2. VISCOSITY CHANGES IN THE PROTOPLASM OF CUMINGIA EGGS DURING THE INTERVAL BETWEEN FERTILIZATION AND THE FIRST CLEAVAGE

The ordinates represent arbitrary units of viscosity. When the protoplasm is most fluid it has a viscosity equal to that of the unfertilized egg. Thus it is approximately twice that of water if the granules are disregarded, or four to six times water if the granules are included (see above). In the figure the *V*'s indicate a viscosity less than the value for y at that point, the inverted *V*'s indicate a greater viscosity. The length of the *V*'s and inverted *V*'s is also significant. Points on Chambers' curve are indicated by *C*'s.

The cycle of viscosity changes during the period between fertilization and cleavage in the sea-urchin egg has frequently been confirmed by workers at Woods Hole who have used the centrifuge. As a matter of fact for a number of years classes in the Physiology Course at the Marine Biological Laboratory have duplicated the results outlined above.

With the egg of *Cumingia* it is possible to obtain more exact data concerning the viscosity changes during mitosis. This egg is in the metaphase stage of the first polar body division when it is shed into the sea-water. At this time the egg protoplasm is highly fluid. The viscosity increases just before the completion of the first maturation division, drops again as the second polar spindle develops, rises as the second polar body is given off, to decrease again with the appearance of the amphiaster of the cleavage spindle. The complete curve of the viscosity changes in the *Cumingia* egg is given in figure 2, which is taken from a paper published in 1921 (Heilbrunn, 1921). In this graph, the *X* axis represents time in minutes after fertilization, the total time being fifty minutes. This was regarded as a standard time interval, and observations made on eggs which required a longer or a shorter time for the period between fertilization and cleavage were plotted by making a correction in the time. Thus if the cleavage time for a lot of eggs was sixty minutes instead of fifty, each observation on this lot of eggs was assumed to take place at five-sixths the number of minutes after fertilization at which it actually did occur. The ordinates in figure 2 are arbitrary viscosity units. As a matter of fact, as has been shown by more recent work, they do really represent a fairly close agreement with the absolute viscosity units in terms of water. Each of the symbols of the figure represents a centrifuge test. The *V*'s indicate that the viscosity was less than the value of *y* at that point, the inverted *V*'s indicate that it was greater. For further details regarding the meaning of the symbols used the reader is referred to the original paper.

The viscosity changes in the egg of *Cumingia* are very striking. The protoplasmic viscosity increases at least five or six hundred per cent in a very short time. The egg of the sea-urchin *Arbacia* is somewhat larger than the egg of *Cumingia*. Its viscosity changs are not as pronounced as those of the latter, the viscosity increase probably not being more than three or four hundred per cent. The egg of *Nereis* is larger still and contains a larger proportion of yolk materials, and a relatively small spindle. In this egg the viscosity changes are to some extent obscured and are rather more difficult to follow. However the same cycle of viscosity changes was found to occur. It is apparent that with the increase in the size of an egg and the relative decrease in the size of the spindle in proportion to the total mass of the cell, the viscosity changes during mitosis become less evident. But even in an egg as large as the frog egg, Ödquist (1922) has been able with the centrifuge to detect periodic variations in the viscosity of the proto-

plasm. These he regards as in essential agreement with my results on *Cumingia* and *Arbacia* eggs.

It is interesting to find moreover that exactly the same sort of cycle of viscosity change during mitosis occurs in a widely different type of cell. Thus Zimmermann (1923) in the alga *Sphacelaria fusca* was able fully to duplicate the results obtained with *Arbacia* and *Cumingia* eggs. He states,

> Wir können also in der Prophase eine Zunahme der Plasma-Viskosität beobachten, in der Metaphase eine rasche Abnahme und nachher wieder eine allmähliche Steigerung. Die Versuche stimmen überein mit Zentrifugierungsversuchen, die amerikanische Forscher, u. a. Heilbrunn (1920), an tierischen Zellen unternommen haben und ferner mit meinen Beobachtungen über die Brownsche Bewegung.

Zimmermann used the centrifuge method.

The periodic cycle of viscosity change during cell division is almost certainly related to the rhythmic changes in susceptibility of dividing cells to various toxic substances. Such rhythmic changes in susceptibility were described by Lyon many years ago (1902, 1904), and they have been interpreted by many biologists as indicating a rhythmic change in the permeability of the cell during mitosis. But the cell is not only periodically more and less susceptible to chemical substances, it also goes through a cycle in which it is more and less susceptible to cold, to heat, and to light (Schleip, 1923; Ruppert, 1924). Such rhythmic susceptibility to physical agents can scarcely be interpreted on the basis of a periodic change in the permeability of the plasma membrane of the cell, for the changes in permeability could scarcely affect the entrance of heat or light. As a matter of fact there is no valid reason for associating the cycle of susceptibility changes with an almost purely hypothetical cycle of permeability changes, especially inasmuch as the actual facts regarding viscosity change offer just as simple an explanation.

There is strong evidence that the viscosity changes which occur during cell division in the sea-urchin egg are an essential part of the process. This has been clearly shown by the author's work on artificial parthenogenesis (1915, 1925a). Of the many different types of treatment which are capable of inciting the egg to divide, each and every one produces an increased viscosity in the protoplasm. There are no exceptions. Moreover, it is only necessary to prevent viscosity change in order to prevent mitosis from occurring (Heilbrunn, 1920a and b). Such prevention of viscosity change need not produce any decided injury in the protoplasm, the egg is anesthetized. A fuller discussion of the subject of anesthesia will be given in the next section. At this point it is only necessary to state that no matter how viscosity change is prevented, the mitotic phenonena are always inhibited.

It has thus been demonstrated not only that viscosity changes occur during mitosis, but that these changes are important and must be considered in any theoretical interpretation of the process. It is undoubtedly true that viscosity changes occur in the course of other vital phenonena, but in no case is the evidence as complete as it is in the case of cell division.

In 1919, Leblond (1919 a and b) published observations on viscosity changes in freshwater algæ. He studied the Brownian movement in the protoplasm of these cells, and he concluded that the protoplasm might change from a gel to a sol, and back again. Leblond states that the gel-sol transformation occurs only when the alga cell changes from a period of rest to a period of functional activity such as growth, division, or reproduction. A similar change is found in various

marine ova. In the primary oocytes of various forms, such as *Nereis* for example, the protoplasm is quite rigid. The breakdown of the germinal vesicle introduces a period of activity and the protoplasm becomes much more fluid.

In *Amœba*, Bayliss (1920) showed that electrical stimulation produced a cessation of Brownian movement. This he interpreted as being the result of a gelation in the protoplasm.

An interesting case has recently been described by Weber (1925d). From a study of the plasmolytic behavior of the guard cells of the stomata of *Vicia faba*, he concludes that the viscosity of the outer layer of the protoplasm of these cells is high when the stomata are open and low when they are closed.

One of the most interesting questions in the whole field of physiology is the question as to the nature of the physical changes that occur when a muscle contracts. In the past some physiologists have held the view that a muscle is coagulated or gelated during activity, others have fought for the opposite theory and have regarded muscular contraction as being primarily due to the swelling of a gel. Recently Gasser and Hill (1924) have attempted to determine which type of viscosity change actually does occur when a muscle contracts. Attaching a muscle to a steel spring, they have studied the effect of the muscle in damping the vibrations of the spring, and from this have calculated the viscosity. They conclude that the viscosity of the muscle increases markedly during contraction, but unfortunately there seems to be a question as to whether their results might not be open to some other interpretation.

It is obvious that the work on the viscosity changes during protoplasmic activity has just begun. In the future, and probably in the near future, many other types of cells will be studied in the hope of finding some correlation between the colloidal behavior of the protoplasm and its vital behavior. Furthermore, study will be directed toward a better understanding of the sequence of chemical and physical phenomena which are involved in viscosity change.

VISCOSITY CHANGES DURING ANESTHESIA

It has already been stated that the viscosity changes in protoplasm are closely related to at least some of its important functions. No better proof of this statement could be given than some of the recent knowledge regarding anesthesia.

In the first place it should be pointed out that certain anesthetics are almost universal. A low percentage of ether will stop various types of cellular activity throughout almost the whole breadth of the animal kingdom. From this extremely important fact it may be concluded with some show of reason that the essential mechanics of protoplasmic activity are similar in widely different types of the living substance. Ether, in much the same concentration, will stop the movements of a cœlenterate, will prevent the division of a sea-urchin egg, block the passage of an impulse along a nerve, or prevent the contraction of a muscle. It thus is capable of inhibiting many types of physical activity of protoplasm, although apparently it has less influence on the chemical activity of the cell.

What is the nature of the ether effect? Some authors have supposed that the primary action was on the permeability of the plasma membrane, although there has never been an agreement as to whether ether caused an increase or a decrease of permeability. As a matter of fact, most of the evidence as to the relation of ether to permeability is untrustworthy (Heilbrunn, 1925c).

It was shown by Heilbrunn in 1920 that ether and various other fat solvents when present in dilute concentration lower the viscosity of sea-urchin egg protoplasm. In higher concentration they cause an irreversible gelation and death. In plant cells as well as in animal cells, dilute ether solutions cause a decrease in protoplasmic viscosity, whereas more concentrated solutions produce an increase (Heilbronn, 1914, 1922; Weber, 1921b, 1925b). This is true for cells of *Vicia faba*, for slime molds, for *Spirogyra* cells, for cells of the leaf of *Elodea*, and the stalk of *Callisia repens*. In the case of the sea-urchin eggs ether and other fat solvents act as anesthetics only when they lower the viscosity. Weber also favors the view that the concentrations of ether which lower viscosity are the anesthetic concentrations for plant cells. Certainly they would inhibit cell division. On the other hand a decrease in viscosity, or in other words a heightened fluidity, could scarcely be expected to prevent protoplasmic streaming. This is prevented by higher concentrations of ether, concentrations great enough to produce gelation. The only difference between plant and animal protoplasm lies in the fact that in the sea-urchin egg the gelation produced by ether is irreversible, whereas it is reversible in at least certain types of plant cells. This perhaps may find an interpretation in the fact now being presented in another paper (Heilbrunn, 1927) that ether in higher concentrations causes a peculiar reaction in the interior of sea-urchin protoplasm, a reaction which depends on the formation of numerous surface films in the interior of the cell.

The actual amount of the viscosity lowering in sea-urchin egg protoplasm has been measured more or less quantitatively (Heilbrunn, 1925c). In unfertilized eggs 2.5 per cent ether lowers the viscosity to about a half of its original value. In fertilized eggs, there is an even more pronounced effect, the viscosity being reduced to one-sixth or one-eighth of its original value.

If we assume that all physical activity of protoplasm is related to colloidal change, then it might be possible to prevent activity either by preserving a state of low viscosity or by preserving a state of high viscosity. It is indeed quite probable that there are two main types of anesthesia. In the one type the protoplasm is kept in a highly fluid condition, in the other it is held in the state of a gel. These two types of anesthesia can actually be demonstrated in the sea-urchin egg (Heilbrunn, 1920b).

EFFECT OF TEMPERATURE ON PROTOPLASMIC VISCOSITY

In a paper published in 1913, Fauré-Fremiet published viscosity measurements of *Ascaris* protoplasm at different temperatures. Fauré-Fremiet's paper is over three hundred pages long and he does not mention his viscosity experiments in his table of contents or summary. It is not surprising therefore that this paper has been generally overlooked by later workers in the same field. The optimal temperature of *Ascaris* protoplasm is close to 35°C.; as the temperature drops below 35°, the viscosity of the protoplasm progressively increases. Fauré-Fremiet measured the viscosity by noting the length of time necessary for displacement of mitochondria granules when the eggs were centrifuged. He records only five observations. These are shown in the following table, in which the second column gives the time necessary at a centrifugal speed of 2500 turns per minute to throw the mitochondria from one end of the cell to the other.

Temperature	*Time*
35°C.	4 minutes
30°	8 minutes
23°	20 minutes
18°	45 minutes
8°	225 minutes

It will be noted that there is no measurement between 18° and 8°C.

In 1917, F. and G. Weber determined the viscosity of the protoplasm of the cells of bean seedlings (*Phaseolus multiflorus*) by the gravity method. They found that the viscosity increased as the temperature was lowered.

Pantin (1924) found by centrifuging the eggs of *Nereis* at different temperatures that the viscosity decreased as the temperature was raised. He made measurements at only four temperatures, 0°, 10°, 20°, and 30°C.

In 1924, I plotted the temperature viscosity curve for the protoplasm of the *Cumingia* egg. The viscosity is relatively very high at 1°C. But at 2°C. the viscosity of the protoplasm drops to a low value, rising then gradually as the temperature is raised until it reaches a maximum point at about 15°C. At temperatures above 15°C. the viscosity again decreases progressively, until suddenly at about 32°C. a sharp increase in viscosity occurs. Heilbronn (1922) in his study of slime mold protoplasm also found a maximum in viscosity at about 15°C. Both above and below this temperature the viscosity of the protoplasm decreased.

It is possible that such a maximum escaped other workers who failed to make observations at frequent temperature intervals. If one compares the data obtained by Pantin on the *Nereis* egg with the viscosity curve plotted for *Cumingia*, it will be found that three out of the four points determined by Pantin fit very well on the curve as plotted for *Cumingia*, and the fourth point is not so very far off. However there is good reason to suppose that there may be an essential difference between the protoplasm of *Nereis* and *Cumingia*. In the form of *Nereis* obtainable at Woods Hole, the unfertilized egg has a large germinal vesicle and its protoplasm is relatively very viscous, that is to say it is a gel, whereas the protoplasm of *Cumingia* is highly fluid.

In his comment on the viscosity curve in *Cumingia* eggs, Pantin states that

> The curves showing the relation of the majority of biological processes to temperature show no relation to such a viscosity temperature curve. For instance, the curve relating temperature to the rate of beat of the Terrapin heart is a smooth curve gradually reaching zero near 0°C.; if the state of the protoplasm changed with the temperature, as in Cumingia eggs, one would expect the change to be reflected by a deviation from the observed smooth curve, the deviation being maximal where the viscosity was maximal.

As a matter of fact in many cases such deviations do occur at about 15°C. (see Kanitz, 1915). This point has recently been emphasized by Crozier (1924).

EFFECT OF ELECTRIC CURRENT

It has already been pointed out that when an electric current is passed through an ameba, the viscosity of the protoplasm increases (Bayliss, 1921). Bersa and Weber (1922) found for cells of *Phaseolus* that small electric currents caused a sharp increase in viscosity. They determined the viscosity change with the centrifuge method and found it to be at least threefold. When the electric current stopped flowing, the protoplasm returned nearly or completely to its original fluidity.

EFFECT OF X-RAY AND RADIUM

Weber (1923) found no effect of X-ray on the protoplasmic viscosity of two types of plant protoplasm. He radiated *Spirogyra* with 170 "H" units, and then cen-

trifuged five minutes later. There was no observable difference between the radiated material and the controls. Another test twenty hours after the radiation showed an increased viscosity in the treated cells. This Weber interprets as being due to a secondary effect. Weber also radiated seedlings of *Phaseolus multiflorus* with 24 H units. Following such treatment there was no observable change in the protoplasmic viscosity.

Forbes and Thacher (1925) exposed fertilized *Nereis* eggs to radium and found a slight decrease in protoplasmic viscosity. They used the centrifuge method.

THE ACTION OF VARIOUS CHEMICALS

The action of fat solvent anesthetics has already been considered. Various types of protoplasm have their viscosity lowered by weak concentrations of fat solvents. Higher concentrations produce gelation.

Apparently all substances which have a marked effect on the life of the cell produce decided changes in protoplasmic viscosity. All living cells require the presence of salts for their existence. Life doubtless began in the ocean and all forms of protoplasm have a close relation to the salts of their environment. Long ago physiologists showed that these must be properly balanced. Sodium or potassium chloride alone is usually toxic, calcium chloride alone is likewise toxic. Only a combination of sodium or potassium chloride on the one hand and calcium or magnesium chloride on the other gives conditions favorable for life. Doubtless the specific effects of these various salts are many. One important effect is their influence on the viscosity.

In 1923 I showed that in sea-urchin eggs and in *Stentor* sodium chloride tends to increase the viscosity of the interior protoplasm. Magnesium and calcium chloride have the opposite effect. Similar results have apparently been obtained by Scarth for plant protoplasm (Scarth, 1924b). After long exposures to calcium the initial liquefaction may change to a coagulation (see Heilbrunn, 1925b). Aluminum acts like calcium but much more powerfully. Experiments with calcium are complicated by the fact that this salt plays a specific rôle in the surface precipitation reaction (Heilbrunn, 1927).

Both acids and alkalies coagulate protoplasm, at any rate they cause a pronounced increase in the viscosity. The effect of carbon dioxide has been studied by Jacobs (1922). He used the centrifuge method, and studied the effect of bubbling carbon dioxide through the surrounding medium on the protoplasmic viscosity. In protozoan cells, *Paramecium* and *Colpidium*, he found that short exposure to carbon dioxide caused liquefaction, whereas longer exposure caused coagulation. Jacobs also experimented with sea-urchin eggs, and with this material he found only coagulation. However, as he himself points out, his experiments with sea-urchin eggs are very few in number. In occasional experiments with sea-urchin eggs I have found that their behavior is not unlike that of the protozoan cells.

Finally it may be pointed out that many protoplasmic poisons cause gelation in protoplasm. Copper sulphate, mercuric chloride, formalin and various other poisons have been tested on sea-urchin eggs. All of them produce a rapid and complete gelation.

The present review of the literature on protoplasmic viscosity has been hastily prepared. It was written at the request of the editor of this journal more or less against the writer's better judgment. It is his hope that the survey of literature

which he has presented has not been too incomplete. There is indeed no claim for completeness. The literature on the viscosity of protoplasm is widely scattered, and some of it has doubtless been overlooked.

In preparing the review it was felt worth while to stress the work which in the sober judgment of the writer gave indication of being accurate. Physical measurements in biology, at least in the study of protoplasm, are of comparatively recent date. The average biologist is scarcely trained in their evaluation. He is apt to weigh equally the statements of a worker who estimates viscosity by looking at a cell with those of other workers who make an honest effort to obtain reliable objective measurements. In whatever criticisms he has made the author has been actuated solely by one motive, namely that of discovering the truth in so far as possible.

In the future the physical study of protoplasm, if it is to prosper, must be conducted by workers who are willing to submit both their own work and that of others to close scrutiny and criticism. Accurate measurements of the physical properties of living matter are not easy to make. There are numerous pitfalls for the incautious seeker after quick discoveries. Only by a proper realization of the difficulties and a willingness to face them can progress be made.

The fact that within the last ten or twelve years a real beginning has been made is decidedly encouraging. We now know a considerable number of definite, well established facts in regard to the viscosity changes which may occur in living matter. It is in the hope that a review of what is now known may help in the accumulation of additional and more accurate data, that this material is herewith presented.

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ABNORMAL SEXUALITY IN ANIMALS

II. PHYSIOLOGICAL

By F. A. E. CREW

Animal Breeding Research Department, University of Edinburgh

IN A previous issue of this Journal (Vol. I, No. 3, July 1926, pp. 315–359), there was discussed Abnormal Sexuality, I. Intersexuality; A. Due to disharmony in the distribution of the genotype; B. Due to disharmony in the composition of the genotype. The subject is continued herein.

C. Intersexuality due to the overriding of the genotype

For purposes of discussion, it is proposed to refer to this category as physiological intersexuality. In certain of the cases cited as examples, there must remain a doubt as to which of these arbitrarily chosen classes, genotypical and physiological, the particular example belongs until more is known of the genetics of the form, and of the mechanism concerned in the processes of developmental physiology. It is solely in order to ease discussion that this legitimate rigid classification has been adopted.

1. Through the action of a foreign biochemical agency. (a) The case of the bovine free-martin. Twins are not uncommon in cattle. They may be two perfectly normal males, two normal females, one male and one female, both normal, or one normal male and an individual which is abnormal in its sex organization, and in this alone. The questions to be considered are these: (1) Are twins in cattle identical or fraternal, i.e., do they have their origin in one and the same zygote or in two separately fertilized ova? If they are identical, then the sexually abnormal individual must be a genotypic male, i.e., of the same sex as its normal co-twin; if the twins are fraternal, then the abnormal individual may be either male or female, genotypically. (2) How is this abnormality produced?

The earlier investigators who had examined the case of the bovine free-martin had regarded the abnormal individual as an identical twin, and, therefore, as a genotypic male (Spiegelberg, Hart, Bateson, and Cole). However, in all save one of 126 cases of twins in cattle thoroughly examined by Lillie, two *corpora lutea* were found. The inference is that twins are almost invariably binovular in this animal, since two ova are concerned in such pregnancies; for when an ovum is discharged from the mammalian ovary, the scar tissue of the ruptured follicle remains as a glandular structure known as the *corpus luteum*.

The two fertilized ova pass into the bicornuate uterus and become attached to the uterine mucosa. As the zygotes increase in size, the embryonic membranes of the two fetuses meet to adhere and in many cases to fuse. If such fusion occurs, an anastomosis (intermingling) of their blood vessels can result, so that a common vascular intercommunication may become established.

Thus the situation arises in which the sex-hormone of each developing individual is at liberty to pass into the tissues of its co-twin. The sex-hormone is the instrument which models the sex organization alone. The internal secretions of the pituitary, thyroid, adrenal, and so forth, can also pass from each individual to the other, but these are mainly concerned in the general and not in the special development of the individual and will be alike in both twins.

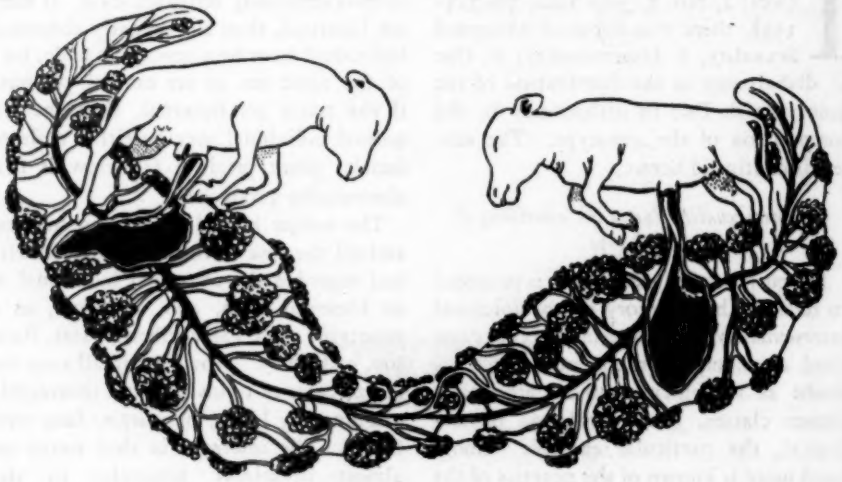

FIG. 15. THE BOVINE FREE-MARTIN (AFTER LILLIE)

But if it so happens that the twins are bisexual (and a study of the sex-ratio in cattle shows that by the laws of probability three possible sex combinations in twinning must occur in the proportion of 1♂♂ : 2♂♀ : 1♀♀, and that the twin combination that includes a free-martin must be regarded as male : female), and if fusion of the chorions occurs, and further if a vascular intercommunication becomes established, as it does in seven cases out of eight, the sex-differentiation of both individuals will be directed by that sex-hormone which is exhibited earlier or which is more potent. The testis becomes differentiated at an earlier stage of development than the ovary, and so the sex-hormone of the male is liberated before that of the female. The male sex-hormone is liberated before the embryonic gonads of the genotypic female have undergone differentiation into ovaries; such differentiation is prevented and so there is no question of a competitive action between male and female sex-hormones. The female twin (i.e., genotypic female) will thus pursue her sex-differentiation under the direction of the male sex-hormone of her co-twin and will, therefore, come to possess more or less completely the organization of the male. The assumption of the male characters in the case of the fetuses examined is imperfect; the external genitalia are of the female pattern, the internal organs of reproduction more or less completely male. The end-result will have a relation to the time of exhibition of, and to the efficiency of, the male sex-hormone. The variation in the size of the testes of the male co-twin

and of the extent of vascular intercommunication seems to point to the conclusion that the amount of the hormone is not a significant factor in the production of a free-martin but that there is a minimum stimulus and the reaction is of the "all-or-none" type. It is seen that the tissues of the genotypic male respond completely to the stimulus of the testicular sex-hormone, whereas those of the genotypic female fail to do so. Now it is known that genotypic male and female tissues are to be distinguished by differences in their chromosome content, and it is reasonable to assume that because of these differences they differ also in their physiological constitution. It is probable, therefore, that though both kinds may be capable of responding to the stimulus of one and the same sex-hormone, they will respond differentially. The male cotwin develops testes because he is a genotypic male and becomes a phenotypic male because he develops testes. His embryonic gonads became testes because their differentiation was pursued under the direction of the male-differentiating reactions elaborated by the interaction of the genes in the sex-determining gene complex. The sex-hormone elaborated by the testes, passing into the body of the genotypic female, swings her sexual differentiation in the male direction, but the swing is not complete, because the sex-hormone of the testis is not equivalent physiologically to the male-differentiating substances elaborated by the sex-determining factors, and because her tissues are constitutionally different from those of the genotypic male. The degree of development of the mammary glands of the free-martin is as that of the immature female. This fact is not without interest for it is known that mammary development is not an infallible indication of the presence of ovarian tissue: in the human subject many cases of considerable development of the mammæ in the male have been recorded. It is to be noted that in the case of twinning involving one normal male and one normal female, there is no vascular anastomosis.

Moore (1921) has shown that it is possible in the rat to remove one testis and replace it by an ovary and that following this, both testis and ovary continue to produce functional gametes. It is possible, therefore, to secure a balance of the male and female hormones in one and the same individual and so to produce an artificial hermaphrodite although such an individual could not function both as a male and a female. Sand (1923) has demonstrated the effects of simultaneous implantation of ovary and testis into an infantile castrated guinea pig and of implantation of ovaries into the testes of rats and guinea pigs and has shown that artificial hermaphroditism with pronounced bisexuality of the psycho-sexual characters could be produced associated with the synchronous development of both male and female sexual organs. These observations support the suggestion that in the free-martin there is no antagonism of sex-hormones, but that the male sex-hormone is liberated first and inhibits the proper development of the ovary. As has been stated, intersexuality is comparatively common in the goat and the pig. It is probable that many of these cases are free-martins, for Keller (1920) and Hughes (1927) have shown that the same kind of placental anastomosis can be demonstrated.

That this is the correct interpretation of the case of the free-martin seemed to be supported by the work of Minoura (1921), who claimed to have produced an equivalent condition in the chick. A small window was made in the shell of a seven to nine days' incubated fertile hen's egg,

and on to the vascular area of the chorio-allantoic membrane were grafted small pieces of testis or of ovary from embryo chicks and adult fowls. In a certain proportion of cases the operation did not interfere with the further development of the chicks and the grafts, it was claimed, showed modification in their sex organization; fourteen individuals were affected as a result of ovarian grafts and sixteen by testicular. In the case of male embryos which had received an ovarian graft, modification in the direction of the female condition was seen, the right testis being smaller than the left (in the female the right gonad atrophies and disappears), while the Müllerian ducts persisted instead of atrophying as in the normal male. In females grafted with testicular tissue, the right and left ovaries were equally developed and the male genital ducts persisted. In this way, Minoura claimed to have produced "free-martins" of both sexes. If this be so, then the mechanism of sex-differentiation in birds would appear to be identical with that in mammals, the critical point being the differentiation of a gonad of one or the other type which proceeds to elaborate a specific internal secretion. It is not clear, however, whether Minoura's results might not have been due to a simple retardation of sex-differentiation by operative interference. Anyone who is well acquainted with chick embryology knows that there is a very great variation in the processes of sex-differentiation. Moreover, Greenwood (1925), and also Kemp (1925), repeating this work even more critically and more extensively, have completely failed to confirm Minoura's conclusions.

It is seen that in the mammal the most distinctive sex-dimorphic characters are the secondary gonadic and for the development and maintenance of these the presence and action of functional gonadic tissue is necessary. The physiological action of gonadic tissues of the opposite sex causes the further development of incompletely differentiated structures to follow the direction appropriate to that sex and so far as is morphogenetically possible, renders the individual intersexual. These conclusions are fully supported by the results of experimental intersexuality (Steinach, 1913; Sand, 1923; and Moore, 1919).

All the results of experimental embryology indicate that in the differentiation of the sex-equipment two phenomena are to be distinguished, (1) the development of the embryonic architecture, (2) the differentiation of the component structures during further growth, and the attainment of specific form under the direction of the physiological action of the "hormones." The timely production of these hormones is to be regarded as the function of the genes resident in the chromosomes. In the insect (and physiologically similar forms) the products of metabolism that guide the development of tissues towards definite form and structure are present within the individual cell and are elaborated there almost if not completely independently of the rest of the body. In the mammal the control of differentiation is removed from the genotype of the individual cell to become the especial function of the glands of internal secretion. Intersexuality in the insect is the direct expression of an unusual genotype, in the mammal the genotype can be overridden.

The fecund tortoiseshell tom-cat is indeed a rarity, and many have been the attempted interpretations of this undoubted fact. Little (1910) and Doncaster (1910) have each suggested that the infecund tortoiseshell tom is a feline free-martin, but Bamber (1912) who examined

a series of multiple births, found no indication of chorionic fusion or of intervascular communication.

(*b*) *The reciprocal free-martin in the opossum.* Hartman and League (1925) described a sex-intergrade opossum which was bought in as a male, for so it appeared, but which was found, on closer inspection, to have skin folds, simultating a pouch, better developed than in the normal male. The penis was of normal size and structure, and the scrotum was well formed but empty; the head, previously described as being of the female type by Hartman, was of the male type. The internal genitalia consisted of an accessory sexual apparatus of the infantile female type. All the parts of the Müllerian duct were present as also were the vagina, lateral vaginal canals, uteri, and Fallopian tubes. The glands were infantile in dimensions and in structure. The round ligaments were normal, and therefore large in proportion to the rest. There were no vasa deferentia and the Wolffian duct derivatives were absent. The gonads were in the position of the ovaries and were very small; their histological structure was somewhat indefinite.

Though the authors could not finally establish their contention that this abnormal individual was a male rendered abnormal whilst in utero by the action of the sex-hormone of a normal female cotwin, they present certain evidence in favor of this interpretation. They compare the specimen with the bovine free-martin, so:

Free-martin	*Specimen*
Genotypic female.	
Female habitus.	Male habitus.
Female external genitalia.	Male external genitalia.
Udder present.	Pouch present, but defective.
No scrotum.	Scrotum.
Clitoris.	Penis.
Müllerian duct repressed.	Müllerian duct developed.
Wolffian duct developed.	Wolffian duct inhibited.
Male vas.	Gärtner's duct only.
Inguinal ligaments.	Round ligaments.
Gonad more like testis.	Gonad more like ovary.
Pflüger's tubes absent.	Pflüger's tubes absent.
Sex cells destroyed.	Sex cells absent.
Sex cords become tubules.	Sex cords as in female.
No Graafian follicles.	Anovular Graafian follicles.
Rete as in testis.	Rete as in ovary.

Whether the interpretation put forward is the correct one or not cannot be stated until further and fuller examination of similar cases has been made, but if it is granted that there is a chorionic anastomosis and that the ovary exerts its physiological activity before the testis, and that in the opossum there are secondary gonadic characters, there is no inherent flaw in the hypothesis.

(*c*) *The case of Bonellia.* The marine worm, *Bonellia viridis*, displays a remarkable degree of sex-dimorphism. The female has a plumpish green body about the size and shape of a plum, and lives under stones or in a hole in a rock, with a long, slender, terminally bifurcated, ciliated proboscis protruding for food-catching purposes. The male is a microscopic pigmy whose internal organs, save those concerned with reproduction, are entirely degenerate, and which lives as a

parasite within the body of the female. The fertilized eggs hatch out as free-swimming larvæ. If a larva settles down upon the sea bottom, it becomes, with few exceptions, and after a short period of sexual indifference, a female; but if by chance or perhaps through attraction it settles upon the proboscis of a female, it becomes a male. The sexual fate of a larva is determined by an accident of position. Baltzer (1914) took larvæ at various periods after they had settled upon a female, but before they had become completely male, and forced them to lead an independent life, and, as a result, he obtained intersexual

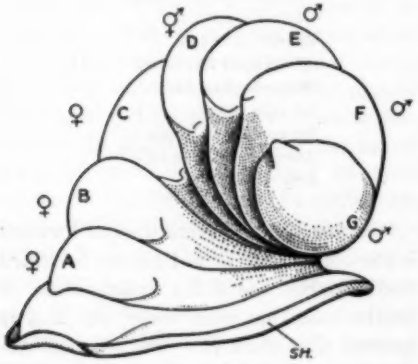

FIG. 16. CHAIN OF CREPIDULA

forms, the degree of intersexuality varying with the length of time the larva had been allowed to remain upon the proboscis of the female. He stained the proboscis of the female with methylene blue and noticed that after the larvæ had settled down upon it for two or three days they also showed the blue stain. He concluded that the larvæ absorb material from the proboscis and that this is responsible for the arrest of growth and the direction of the sexual differentiation. The arrested pigmy male passes from the proboscis of the female into her mouth, and then, after a slight change, he emerges therefrom, and ensconces himself in the reproductive duct through which the eggs pass out into the water.

Baltzer has more recently (1925) shown that weak solutions of the green skin of the female proboscis are poisonous to many of the smaller forms of aquatic life and that the pigmy males of *Bonellia* are extremely sensitive to such solutions, over one in 3,000 being lethal; mere contact with the skin produces no ill effect, it is the absorption of substances that is fatal. The wall of the reproductive duct in which the adult males live has no poisonous action.

The efficacy of purely external stimuli to influence sex-differentiation is also seen in the case of the slipper limpet, *Crepidula*, which, introduced into this country from America, became such a plague to oyster fisheries. *Crepidula* lives gregariously in chains; the free-swimming young settle on older individuals and grow where they settle. Each individual after attaching itself passes through a phase of sexual indifference, next through a male phase, producing spermatozoa, then through a hermaphrodite phase, producing both spermatozoa and ova, and finally ends its days as a female, producing ova only. Gould (1917) found that in *Crepidula plana* the male phase occurs only if the individual settles in propinquity to a larger individual, larger and older, and, therefore, a female; but that if the individual is isolated and prevented from settling upon a female, it passes directly from the neutral to the female phase. A very similar state of affairs is recorded in the case of *Crepidula fornicata* by Orton (1909).

An ingenious hypothesis has been advanced to cover the observed facts concerning *Bonellia*, and in this matter the case of *Crepidula* is very similar. It is suggested that the sex-determining genes produce sex-differentiating substances unde

the direction of which the differentiation of the sex-organization is pursued. Goldschmidt (1923) supposes that in all the individuals there is at first an excess of male-differentiating substance, but that the production of the female-differentiating substance after a time overtakes this. Further, he supposes that the secretion of the proboscis of the female of *Bonellia* has the effect of accelerating the process of differentiation (as opposed to the process of growth), of antedating, as it were, the period during development when sexual differentiation occurs. When differentiation is rapid, the sex organization matures under the influence of the male-differentiating substance; when it is not accelerated, under that of the female-differentiating substance. The mode of sex-differentiation is determined by a varying physiological state in connection with varying environment and secretions from other individuals (fig. 17). Goldschmidt, in a more recent paper (1926), states that in *Bonellia* sex is genetic. Larvae that are genetic females develop into females. Genetic males, developing without parasitism, pass through transitory intersexuality into females. Males that become parasitized develop into normal males if the process of sex-differentiation is completed during the male phase of development; if, however, the female phase sets in before the completion of the sex-differentiating process, such individuals develop as intersexes.

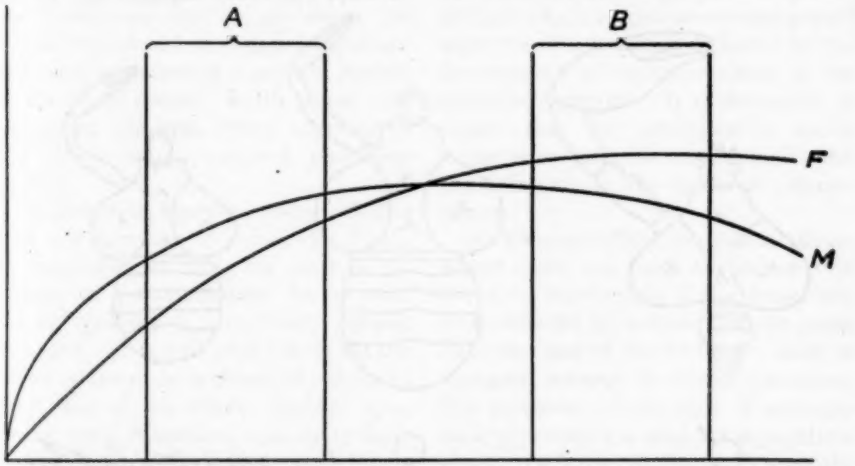

FIG. 17. *A* is the phase of sex-differentiation pursued at a time during development when the male-differentiating substances are effectively in excess; the result is a male type of sex-organization.

B is the same phase pursued under conditions when the female-differentiating substances are effectively in excess; the result is a female type of sex-organization.

The case of *Asterina gibbosa* (Cuénot, 1898) would appear also to belong to this group. At Roscoff every individual is first male and later female; at Banjuls the two phases overlap to a greater extent; at Naples the different types occur irregularly.

2. *Intersexuality through parasitism.* (*a*) *The case of Inachus and Sacculina.* Giard (1887) and later Smith (1906) described in detail the changes that occur in crabs parasitized by *Sacculina* and other parasitic

Crustacea. *Sacculina*, an internal parasite, is a cirriped crustacean, and part of its body projects to the exterior under the abdomen of the crab in which it is living, while root-like processes, which absorb the juices of its host, ramify to all parts of the crab's body, avoiding the vital organs and absorbing nourishment chiefly from the blood. It attacks males and females, and in both it causes atrophy of the sex-glands and consequent sterility. The only effect of this in the female is acceleration in the assumption of the adult sex characters. Parasitized males, however, gradually take on more and more of the female characters, their great claws become smaller and smaller, the abdomen broader, the swimmerets enlarge and become fringed with the hairs to which, in the females, the eggs are attached. Most of the affected crabs die, but in a few the parasite disappears and the reproductive organs are regenerated. In a female a normal ovary develops, in a but partially feminized male, a normal testis, but in a fully feminized male a sex-gland is regenerated, in which both ova and sperm are found: real male intersexuality is produced. Geoffrey Smith (1906), who investigated this problem, found that the blood of the normal female crab differs in chemical constitution from that of the male. It contains fatty substances which are absorbed by the ovaries and used in the production of the yolk of the egg. These fatty substances form an important part of the food of the parasite *Sacculina*. That which *Sacculina* absorbs cannot be used in yolk formation, and as the eggs cannot develop, the ovary degenerates. In the male these fatty substances are present in but small quantities. The parasite demands more and the whole physiology of the male crab is altered to meet this demand; the male thus assumes the female type of metabolism, and consequently the characters of the female.

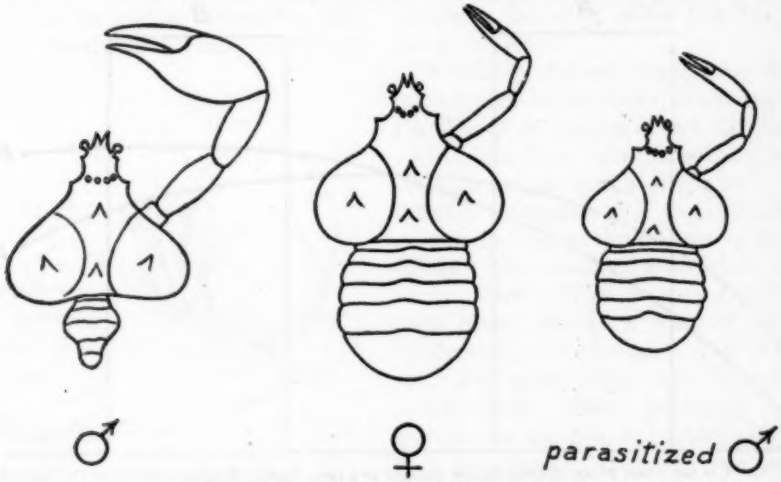

FIG. 18. THE CASE OF *Inachus* AND *Sacculina*

Robson (1911) states that the infection by *Sacculina* induces the maintenance of an abnormal quantity of fat in the liver and blood of the host. This condition resembles that found in normal females and males preparing for the molt, and in sexually mature females, the ultimate

destination of the fat being functionally similar in the case of mature females and the infected crabs. In all probability the ultimate fate of the infected crab is death from starvation, arising from its inability to obtain enough fatty material for itself and its parasite. A pink colored lipochrome is found in the blood of molting and infected animals of both sexes, while a rich yellow characterizes that of the sexually mature female.

The interest of this case is that it permitted Smith to question the validity of the conception that in all forms the gonads functioned as organs of internal secretion, contributing a peculiar product to the blood stream. Smith argued that the gonad, far from adding anything to the blood stream, removed something from it.

Goldschmidt, however, points out that it is not necessary to regard the change in metabolism as being the cause of the change in characterization, for it itself can be regarded as a secondary gonadic character. It is well established that the blood of the male is chemically different from that of the female (Steche, 1912; Geyer, 1913; Manoilov, 1922–1923; Gräfenberg, 1922), while Farkas (1903), Straus (1911), Lawrence and Riddle (1916), and Lipschutz (1916) have shown that the metabolism in the two sexes is markedly dissimilar, but so far it has not been possible to induce intersexuality by means of blood transfusion.

Potts (1906) has shown that the infection of the hermit crab, *Eupagurus meticulosus*, by the cirriped *Peltogaster curvatus* has the effect of diminishing immediately the size of the gonads and of suppressing their functions and that this is effected not only through direct action on the gonads but through interference with the general nutrition of the host. At an early stage of the external parasitism ova appear in the glandular part of the testis; no corresponding changes were traced in the ovary. The male type of characterization became transformed more or less towards the female type under the influence of the parasitism. The same sequence of events was observed also in the parasitized *Eupagurus prideauxi*.

It is to be noted that the development of ovarian tissue in the parasitized male crab is actually a process of regeneration. It is of interest to note, therefore, that in the case of the worm, *Ophryotrocha puerilis* (Braem, 1894), amputation of the genital segments of a female is followed by the development of testicular tissue in the regenerated portion. It is reasonable to assume that the conditions in which regeneration proceeds are responsible for the difference in the mode of differentiation.

(*b*) *The case of Thelia bimaculata.* Kornhauser (1916 and 1919) has shown that the male membracid, *Thelia bimaculata*, when affected by a species of the genus *Aphelopus*, one of the Dryinidæ, early in ontogeny, assumes the female coloration. The pronotum of the male is normally dark brown with a bright orange-yellow vitta on each side; the female is gray, the vitta being only slightly visible. The males of *Thelia* possess in diploid number twenty-one chromosomes, the largest of which is the X-chromosome. The female has twenty-two, two of which are large X's. Females are larger than males and testes develop earlier than ovaries. Parasitized females are not affected. The degree of the change-over in the male depends on the state of development of the parasites in the fifth instar of *Thelia* previous to the final molt. The parasite, a hymenopteron, lays its eggs in the nymph of *Thelia*. Fifty to sixty larvæ result and become full grown during the fifth instar of the host if the egg was

deposited in a *Thelia* of the first or second instar. The larvæ devour everything within the chitin of the host. If, however, they are only partially developed in the fifth instar, they do not kill and the host becomes an adult but modified.

Early parasitism results in retardation of differentiation of external genitalia in both sexes but no transformation. Parasitism of the male is followed by considerable increase in the body size, while in the case of the female it is followed by a slight decrease. Parasitized males assume female-type size of wing, size, pattern, and color of head, size of proboscis, size of legs, size of digestive tube, and size of abdomen. Parasitism of females results in a decrease in body size, but unaltered form, save that the ventral and terminal plates are soft and non-pigmented.

The results of parasitism are not due to the initial effect on the gonad, for in a parasitized male the testes may be normal. The changes are to be associated with change in nymphal metabolism. Male nymphs grow more rapidly, are smaller, darker, and are sexually mature when they become adult, the testes filling the greater part of the abdomen. The male is katabolic. Female nymphs develop slowly and become larger, store fat, and are anabolic.

Parasitism in the male brings about lower oxidation, storage of fat, retarded development, increased size. The external genitalia are laid down too early to be affected.

3. *Intersexuality through gonadectomy.* (*a*) *The case of the mammal.* Pearl and Surface (1915) have described the case of a cow with cystic degeneration of the ovaries which assumed the general appearance of a bull. Rörig (1900) and others have recorded instances of female deer with ovarian disease developing horns or antlers as in the male. The difficulty that exists in such cases as these is that of distinguishing between male and female characterizations on the one hand, and the agonadic form on the other. Tandler and Grosz (1913), for example, have shown that ovariotomy in the cow is followed by the assumption of a characterization which is intermediate between those of the normal male and female.

(*b*) *The case of the fowl.* The interrelationship between gonad and plumage characterization in the fowl can be demonstrated in the clearest possible way by an appeal to experimental gonadectomy and the implantation of gonadic tissues. The results of such experimentation by a considerable number of investigators (Pézard, 1924; Pézard, Sand, and Caridroit, 1924, 1926; Morgan, 1919; Roxas, 1926; Finlay, 1925; Goodale, 1916, 1918; *et alia*) are shown in table 6.

The fact that the presence of both ovarian and testicular tissues in the male is associated with hen-feathering is of interest in connection with the case of the gynandromorph fowl described by Macklin (this Journal, vol. 1, p. 329). It is established (Torrey and Horning, 1925; Cole and Reid, 1924; Crew, 1925; Zavadovsky, 1925; Brambell, 1926; *et alia* that the administration of thyroid extract to cocky-feathered cocks is followed by the assumption of feathers very similar to those characteristic of the hen of the same breed.

Cocky feathering in those cases in which there is sex-dimorphism in the color and structure of the plumage is commonly regarded as trustworthy indication that within the body there is, or was at the time when the plumage was developed, active functional testicular tissues; henny-feathering as an indication that there is, or was, active functional ovarian tissues. Gonadectomy in both sexes is followed, after a molt, by the assumption of a plum-

age which in its coloration is as that of the male of the variety to which the bird belongs, (save that in particolored breeders there is apt to be more white), while the barbules in the distal portions of the feathers of the hackle region are absent as in the normal male, but the plumage is much looser and far more luxuriant in its growth; the plumage characters of the capon and of the poularde are exactly alike. Since this is so, it is commonly argued that the goads exhibit an endocrine function, the ovarian internal secretion possessing the faculty of inhibiting the development of the plumage characters of the male. The fact that in certain breeds the cock is feathered as is the hen, is explained on the assumption that in their functioning, the testes of such a male are endocrinologically equivalent to the ovary of a hen. Such an interpretation is supported by the facts recorded in the above table, if it is assumed that in the case of a bird in which both ovarian and testicular tissues co-exist the internal secretion of the ovary is more potent or is produced in greater quantities than is that of the testes.

TABLE 6

SEX	OPERATION	EFFECT ON PLUMAGE	COMB AND WATTLES
♂	Castration	Becomes as that of a capon (male in color, looser, more luxuriant). If a hen-feathered cock, it becomes as that of a capon	Shrink
♂	Castration + implantation of testis	Remains as that of a cock. If testis from a cock-feathered cock is implanted into castrated hen-feathered cock, he still remains hen-feathered. If testis from a hen-feathered cock is implanted into a castrated cock-feathered cock, he will remain cock-feathered	Remain as those of a cock
♂	Castration + implantation of ovarian tissue	After molting, becomes as that of the hen of the breed and variety to which he belongs	Become as those of a hen
♂	Implantation of ovarian tissue	After molting, becomes as that of a hen, but more ruddy (warmer looking); tail sickles intermediate	Remain as those of a cock
♂	Implantation of extra testicular tissue	Becomes as that of the hen of the breed and variety to which he belongs; tail sickles intermediate	Remain as those of a cock
♀	Ovariotomy	Becomes as that of a capon of the breed and variety to which she belongs	Shrink
♀	Ovariotomy + implantation of ovary	Remains as that of the hen of the breed and variety to which she belongs, so long as implant remains as ovarian tissue	Remain as those of a hen
♀	Ovariotomy + implantation of testis	Becomes as that of a cock of the breed and variety to which she belongs	Become as those of a cock
♀	Implantation of testis	Remains henny, but more ruddy; tail sickles intermediate	Become as those of a cock
♀	Implantation of extra ovary	Remains henny; tail sickles intermediate	Remain as those of a hen

But there are several facts concerning the fowl, which, while emphatically

demonstrating the existence of secondary gonadic characters, do not support the contention that the gonads function as endocrine organs. On the contrary, they point to the conclusion that the relation of gonads and secondary gonadic characters is similar to that which obtains in the case of *Inachus*. In this discussion it is most important to note that the plumage characterization at the time of examination does not necessarily agree with the kind of gonadic tissue within the body at this time. Modification of the gonad does not affect feathers already grown. The plumage characterization is an accurate reflection of the condition of the gonadic tissue at the time when this plumage was developing.

The cocky-feathered laying hen. For example, there is no reason why a laying hen should not be cocky-feathered, or why a functional cock should not have the plumage of a capon.

The cocky-feathered laying hen is to be explained by the fact that at the time when this plumage was developing the ovary had undergone a more than usually complete involution. The bird then for the time being was without a gonad, and as, in the absence of the physiological activity of a gonad the plumage becomes as that of a capon, the plumage of the poularde began its development. Then the ovary resumed its activity, and the effect of its activity was to tighten up the plumage; it came into action, however, too late to model its structure. The present writer has in his possession a laying hen that has been henny, cocky, henny, and is cocky once more. (Lippincott has described a very similar case.) The head furnishings, the attitude, the behavior, have always been as those of a normal hen.

The developmental capon and poularde. The capon-feathered functional male is the result of a delayed action on the part of the testis during development. Such males are extremely common among White Leghorn flocks. Amongst White Leghorn cockerels there will be found three types: one that becomes sexually active at a very early age, with large erect combs and tight plumage and which continually attempts to crow and fight; another that develops as a bird that has been caponized within the first week of its life, its head furnishings remain bloodless, its body grows steadily in size to become long-legged and awkward, while its plumage is long and loose; and a larger class that falls between these two in respect of the attainment of sexual maturity. The breeder usually selects his males for breeding from among this latter class, and these in their turn produce the three types of male. The capon class matures very late and in many cases that have come under the observation of the writer, they have not attained full sexual maturity even in their third year. Examination will show testes equal in size only to those of the male chicken before the secondary gonadic characters have become expressed, and histologically revealing the fact that spermatogenesis is not complete.

Occasionally a developmental poularde is encountered and is in every way similar to the developmental capon save that she is smaller and that her face is distinctly more like that of the hen. She possesses an ovary of normal structure but one that has remained immature. The fact that the female is smaller than the male, even in such a physiologically agonadic specimen, would seem to suggest that perhaps size is largely determined by X-borne genes, these being duplex in the male, simplex in the female.

A case that cannot be interpreted if it is postulated that the ovary and the testis of the fowl elaborate an internal secretion

specifically different is that recently described by Greenwood and Crew (1926). A Brown Leghorn female chick was ovariotomized when four days old and into her body were implanted the chopped up testes of her brother. She became a typical masculinized female with large head furnishings and perfect cock plumage. At the age of seventeen months this bird molted and the new plumage was as that of a normal Brown Leghorn hen. At first it was thought that this was due to absorption of the testis implant and regeneration of the ovary, but, as the head furnishings still remained as those of the male, it was recognized that this could not be the case. Post mortem examination revealed a very small amount of highly degenerate ovarian tissue and a mass of testis tissue much greater than that usually found in a cock of the same size. The right gonad of the female, as is commonly the case in the ovariotomized hen (Domm, 1924; Benoit, 1924; Pézard and Caridroit, 1923 and Zavadovsky, 1922) had become differentiated as a testis in spite of the fact that the genotype of its constituent cells was XY.

At the time of the assumption of the adult plumage, this hen became completely cocky-feathered. If no gonadic tissue had been present during the critical period of the development of this plumage, this could have been cocky in color and structure, but loose and luxuriant as that of the capon. If at the beginning of this critical period, gonadic tissue had been absent or insufficient, the plumage would have started to develop the characterization of the plumage of the poularde, but, if shortly after this gonadic tissue of either kind, ovarian or testicular, had become sufficiently active, then though in its coloration and structure the plumage would have remained unaffected, it would have become tighter and closer to simulate that of the cock. It follows, then, that at the critical time of development of the first adult plumage ovarian tissue was either absent or insufficient, and that following this either ovarian or testicular tissues became physiologically sufficiently active or else that ovarian tissue was either absent or insufficient throughout, but that sufficient testicular tissue was present either at the beginning or else immediately after this. If the bird was a poularde, the plumage of which in the later stages of its development had been affected by the physiological action of ovarian tissue, then after a molt the plumage would have become completely henny, as indeed it did, the head furnishings would have always been as those of a hen of this breed and variety, which they were not, and only ovarian tissue would have been found post mortem, which was not the case. It is reasonable to assume that the second adult plumage was developed under the stimulus of a mass of testis greater than that usually found in a bird of this size. If this is the case, it is necessary to explain the association of abundant testicular tissues and henny feathering. It is to be noted that the testis tissue implanted was not that from such a male as a Sebright with henny feathering: it was from a Brown Leghorn, a breed in which the males are definitely cocky feathered. The facts of the case can be accommodated if it is assumed that ovarian and testicular tissues, in respect of their own individuation, exert demands upon the general economy of the same kind but different in degree, that the functioning of an ovary is physiologically more expensive than is that the testes, and that it is possible to supplement the demands of the testes so that they become equivalent to those of the ovary. This interpretation will explain the association of henny feathering and both ovarian and

testicular tissues as in Macklin's gynandromorph.

The assumption of male secondary gonadic characters of the senile hen. It is by no means uncommon for the senile female bird, wild and domesticated, to assume the plumage characters of the male of the breed and variety to which she belongs. Examination will reveal the fact that associated with this transformation there has been more or less complete destruction of ovarian tissue or a progressively diminishing physiological activity of this. Since, as has been shown, very similar results follow experimental gonadectomy, it is reasonable to interpret the phenomena as the result of pathological ovariotomy. Ordinarily during the succeeding years of the individual's life the oocytes in their growth make certain demands upon the general economy of the individual and maintain a metabolic level of femaleness, but, should the conditions be unfavorable for their growth as a result of the physiological exhaustion consequent upon excessive egg laying or from hæmorrhage or tumor growth, then, in the absence of the inhibitory influence of the growing oocytes, the plumage becomes as that of a capon which closely simulates that of a male of the breed and variety to which the individual belongs. In old hens it is quite common for the head furnishings to increase in size and to become as those of the cock. In such cases it is found that the plumage remains henny. Examination will reveal the presence of degenerate ovarian tissues in which tumor growth has occurred. It would seem that there is a definite relationship between the mitotic activity of the gonad and the size of the head furnishings.

The nature of the right gonad of the hen. Because it has been found that in the cases examined (Domm, 1924; Benoit, 1924; Zavadovsky, 1922) the right gonad of the ovariotomized hen develops into a testis, it has been argued that the hen is a constitutional hermaphrodite, that the right gonad which in the normal female chick ceases its development at or about the sixth to eighth day of incubation, is from its beginning a testis, but that being a testis in the presence of an actively developing ovary, it is not permitted to flourish. It is difficult to maintain such a thesis as this and in the light of more recent work it is far more satisfactory to explain the fact that this incompletely atrophied gonad becomes a testis in most cases following ovariotomy on the assumption that in spite of the fact that its tissues possess the XY type of constitution they are to a very considerable extent ambivalent in respect of their future differentiation and that their mode of differentiation is decided by the kind of physiological environment in which they find themselves. It will be remembered that in the case of the male, the products of the primary proliferation of the germinal epithelium cease their development at about the sixth day of incubation and are replaced by the products of a secondary proliferation which proceeds to become differentiated, in the case of the left gonad, as ovarian tissue. Any subsequent proliferation of germinal epithelium leads, if it leads anywhere, to the production of testicular tissue. Ovarian tissue is developed solely from a proliferation of germinal epithelium that more or less coincides with the sixth day of incubation. It follows that the difference between differentiation into ovarian or into testicular tissues is a reflection of a difference not inherent in the tissues themselves but in the environment in which this more or less ambivalent tissue develops. If and when the internal environment which obtains at the sixth day of incubation can be analyzed and recon-

structed, it will be possible to direct the differentiation of the products of any proliferation of germinal tissue into ovarian tissue. In this connection, it is of interest to note that Greenwood (1925) has found that, though in the majority of cases in which ovarian tissue has been implanted it ceases to develop as such but is replaced by the testicular, in certain cases it can continue its development as ovarian. It is not without significance that in quite a number of female hawks and owls there is a right ovary with normal follicles.

From this it will be foressen that if in a hen the ovarian tissue is destroyed, and if there is a recrudescence of activity in the rudimentary right gonad, or if a proliferation of germinal epithelium occurs, then testicular tissue will become differentiated, perfectly or imperfectly, and the phenotype, so far as plumage, head furnishings, spur, and behavior are concerned, will be transformed from that of the hen to or towards that of a cock. Greenwood (1925) found that no secondary proliferation of sex cords occurred in ovarian grafts in cases of ovarian implantation made later than the fourth day after hatching so that in the case of older fowls it may be expected that no new ovarian tissue will ever develop but that testicular tissue may develop in any female.

4. Intersexuality due to the impress of external agencies (including temperature and salinity). (a) The case of the oyster. In the dioecious species of oysters three intersexual individuals have been recorded. Kellogg (1890) described one in the American oyster, *Ostrea virginica*, and Amemiya (1925) two in the Portuguese oyster, *O. angulata*. These latter two contained eggs and sperm. The eggs were of ordinary size and shape, being similar to those of other dioecious oysters and much smaller than the eggs of species that are customarily hermaphroditic. Since Amemiya found these specimens among animals which had been kept in the tank for a considerable period, he inclines to the conclusion that the intersexual condition was definitely related to changes in nutrition. In this connection, the work of Orton (1921) on the native oyster, *O. edulis*, is of great importance.

In the case of forms such as those species of the oyster in which phenotypic maleness alternates with phenotypic femaleness, it can be concluded that the sex-chromosome mechanism which establishes the genotypic sex of the zygote is invariably overriden by environmental agencies. The deciding factor in the establishment of the sexual characterization of the individual is not the genotype; genotypic male and genotypic female alike are molded in their development by forces outside themselves. Spärck (1925), as the result of his extensive study of the native oyster (*O. edulis*), is led to the conclusion that it is the temperature of the medium in which the oyster lives that decides its sexual architecture through its direct effects upon the rate of general development, and not indirectly through its effects upon the numbers and general condition of the forms of life which constitute the food of the oyster.

It is not without significance that, as Weisensee (1916) has shown, certain species of *Anodonta* become hermaphroditic when living in isolated stagnant waters but are typically bisexual when in running water. It is probable that the instance of intersexuality recorded by Cuénot (1891) in the holothurian *Synapta inhaerens* and in the asteroid, *Asterinia gibbosa* (1898), and by Mortensen (1920) in certain ophiurids as well as the case of *Patella vulgata* to which reference is made below are all illustrations of the overriding of the

genotypic sex of both male and female zygotes by such agencies as temperature, salinity, or abundance or otherwise of nutriment.

(*b*) *The case of the limpet.* Gemmill (1896) described some cases of hermaphroditism in the limpet, *Patella vulgata*, in which the sexes are normally separate. The genital gland, ovary or testis, is single, and its products pass into one of the kidneys and reach the water by means of the kidney duct. No special genital ducts, no copulatory apparatus, and no secondary sexual structures appear to exist. In the limpet it is only when the individual has assumed its adult form that the generative gland begins to differentiate into ovary or testis. A number of specimens (3 in 250) were found in which the gonad was mixed, and its contents showed not only ripe—olive green—ova, but sperm—yellow—, segmented ova, and even ciliated freely moving embryos. Ovarian and testicular tissues in the gonad were not separated, but merged one into the other. The relative amount of ovarian and testicular tissue varied in different cases. The percentage of females (67) in a population of *Patella*, is independent of level, and consequently independent of time of feeding, since the high level are under water for a shorter period of time. Gemmill suggested that nutrition does not decide sex in this case because the ovary and testis are alike in bulk and there is no sexual ornamentation. These are insufficient reasons for such a conclusion. The work of Orton (1921) on this form is of great interest.

Nachtsheim (1923) records that in females of *Carausius* (Hymenoptera), the development of which proceeded at 25°C., the ventral portion of the thorax took on the red coloration characteristic of the male.

It would seem to be an established fact that there are three classes of animal: (1) those in which the genotype finally and irrevocably determines the sexual phenotype, the sexual characters being secondary genotypic characters; (2) those in which the sexual characters are a combination of secondary genotypic and secondary gonadic; and (3) those in which the action of the genotype is readily and usually overwhelmed by certain environmental agencies under the direction of which alone the sexual characterization is unfolded.

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THE PALÆOMORPHOLOGY OF THE HUMAN HEAD: TEN STRUCTURAL STAGES FROM FISH TO MAN PART I. THE SKULL IN NORMA LATERALIS[1]

By WILLIAM K. GREGORY

American Museum of Natural History

THE human head is always of special interest to anthropologists, who have measured and classified its varieties all over the world. But what is the human head, where did it come from and by what steps did it arise? The purpose of the present paper is to trace, so far as possible from available evidence, ten structural stages in the evolution of the vertebrate head from fish to man.

MORPHOLOGY OF THE SKULL IN FISH

In the Lower Devonian rocks of Great Britain, Russia and Canada, one finds the remote ancestors and relatives of the lobe-finned or crossopterygian fishes and of the dipnoans. The intensive studies of Pander, Traquair, D. M. S. Watson, Bryant and others in the structure of these fossil fishes have led to the now widely accepted conclusion that while neither the lobe-finned nor the dipnoan fishes are the direct ancestors of the land-living vertebrates, both are closely related to the still undiscovered tetrapod stem. On the whole, it is agreed, the lobe-finned fishes of the Devonian give us a fairly close picture of the morphology of the skull in the stage immediately preceding the emergence of swamp and land-living vertebrates.

The head, even of these primitive fishes, is already a highly developed structure. The skull consists essentially of internal or endocranial elements overlain by ganoine-covered surface bones, the latter being of the same nature as the scales on the body. The endocranium includes first, the bony capsules surrounding the nose, eyes and internal ears, and secondly, the bony trough that encloses the brain itself. As to the evolutionary history of the endocranium, considerable progress has been made in recent decades, but the present and following papers are concerned principally with the history of the dermocranium or surface skull.

In these primitive fishes (fig. 1, I) the dermocranium included the following series of elements: (1) roof bones on or near the mid-dorsal line, covering the nose, eyes, pineal organ and hind brain; (2) the maxillary series, including the premaxillæ and maxillæ of the upper jaw and the dentary or inferior maxillary of the lower jaw; (3) the circumorbital series of five plates around the eye; (4) the temporomandibular series; in the skull these plates, consisting chiefly of the squamosal and quadratojugal bones, cover the temporal region; they protect the upper jaw muscles and the back part of the primary upper jaw or pterygoquadrate. In the lower jaw this series includes the plates lying immediately behind and beneath the dentary, namely, the infradentaries or splenial, post-splenial, surangular; (5) the opercular series, covering the branchial chamber and the floor of the mouth.

[1] Read before Section H., Amer. Assoc. Adv. Sci., Philadelphia, Dec. 30, 1926.

AMPHIBIANS

The oldest known stage of the tetrapods or higher vertebrates is that of the amphibians of the Lower Coal Measures of England. Described very imperfectly by earlier authors, they have recently been restudied and redescribed by Professor D. M. S. Watson of the University of London, in a memoir of the greatest importance to all students of the evolution of the vertebrates. Watson, in common with other palaeontologists, regards these embolomerous amphibians as being practically the stem group of all higher vertebrates. In the arrangement of the surface bones of the skull these early gill-bearing forms (fig. 1, II) go far toward bridging the gap between fishes and amphibians. In fact they have inherited all the fundamentally piscine arrangement of the bones of the dermocranium described above, so that one may easily recognize most of the roofing bones, the maxillary series, the circumorbital series and the temporomandibular series. Only the opercular series of the fish skull has disappeared, probably in connection with the change in habits. There is nothing unusual about bones dwindling away and disappearing as we pass from older to later types. The late Professor Williston of the University of Chicago brought forward much evidence showing that this reduction in number of elements, together with further differentiation of the remaining elements, is the normal course of skull evolution in vertebrates. Hence the absence in the oldest amphibians of the whole series of opercular and gular plates is no bar to the derivation of the amphibians from fishes. The place where the chief opercular bone formerly was is occupied in the earliest amphibians by a large notch, commonly called the otic notch. The skin covering this region was already beginning to function as a tympanum or eardrum.

REPTILES

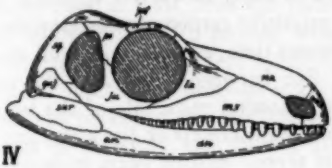
The third stage is represented by a very primitive land-living form (*Seymouria*) from the Permo-Carboniferous beds of Texas (fig. 1, III). This is technically classed as a reptile but retains many pronounced amphibian characteristics throughout the skeleton. In this highly important form as studied by Cope, Broili, Williston, Watson and others, the otic notch is still large and the intertemporal and supratemporal bones are still retained. The maxilla, as in fishes, does not extend upward on the side of the face, while the lacrymal is elongate and reaches from the orbit to the anterior nares.

The fourth stage is represented by one of the more primitive of the theromorph reptiles of the Permian of Texas (fig. 1, IV). Here the maxilla is beginning to grow upward, while the lacrymal has lost its extension to the nares. In the temporal region of the preceding stages the jaw muscles had been covered with an outer shell of bone, but in this form the

FIG. 1. TEN STRUCTURAL STAGES FROM FISH TO MAN. THE SKULL IN NORMA LATERALIS

I. Rhipidistian fish, Devonian (essentially *Rhizodopsis*). After Traquair, Watson.
II. Embolomerous amphibian (*Eogyrinus*), Lower Carboniferous. After Watson.
III. Primitive cotylosaurian reptile (*Seymouria*), Permo-Carboniferous. After Broili, Williston, Watson.
IV. Primitive theromorph reptile (*Mycterosaurus*), Permo-Carboniferous. After Williston.
V. Primitive gorgonopsian reptile (*Scymnognathus*), Permian. After Broom.
VI. Primitive cynodont reptile (*Ictidopsis*), Triassic.
VII. Primitive marsupial (*Eodelphis*), Upper Cretaceous. After Matthew.
VIII. Primitive primate (*Notharctus*), Eocene.
IX. Anthropoid (Chimpanzee).
X. Man.

V



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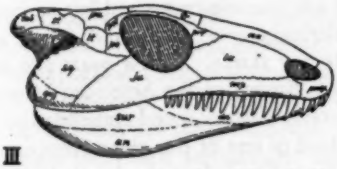

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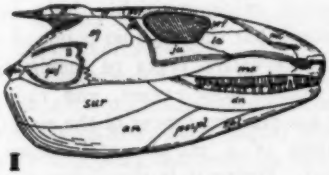

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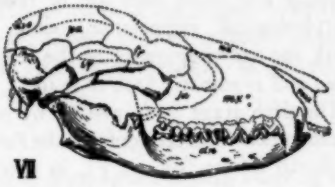
VI



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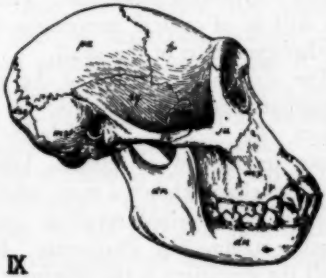
VIII



IX

X

shell had been perforated by a process of natural trephining in such a way that the middle of the area of origin had become thin and finally was perforated by the jaw muscle, while the outer margin of the area had become strengthened and built up into curved bars or arches. This opening and these bars are of profound morphological importance, since they mark the very beginning of the temporal fossa and zygomatic arch of man.

The fifth stage is furnished by one of the mammal-like reptiles from the Permian of South Africa (fig. 1, V), as described by Broom, Watson and other authors. Here the maxilla has already become the dominant element of the face. With the enlargement of the temporal fossa the surrounding bars of bone stand in clearer relief and we recognize easily the beginning of the mammalian zygomatic arch. In the lower jaw, the dentary sends upward and backward a strong process which overlaps the surangular. This process will be of critical importance in the further evolution of the skull.

The sixth stage is one of the smaller cynodonts from the Triassic of South Africa (fig. 1, VI). Technically these forms are classed as reptiles, but their whole skeleton shows a most interesting mixture of inherited reptilian and prophetic mammalian characters. In the skull the maxillary is the dominant facial element and the temporal fossa is almost wholly mammalian in type. The postfrontal bone has disappeared but the prefrontal, postorbital, jugal and lacrymal remain. In the lower jaw the dentary is now dominant and its ascending ramus is now reaching upward and backward toward the squamosal bone. The jaw elements behind the dentary, namely the surangular, angular and articular, together with the quadrate bone of the upper jaw, are much reduced in size in comparison with those of earlier members of the series. The dentition also is almost mammalian, being now thoroughly differentiated into incisors, canines, premolars and cuspidate molars.

MAMMALS

The most progressive cynodonts of the Triassic in many characters approach the more primitive carnivorous marsupials of the present time, especially in skull structure. But between them and the well known placental mammals of the beginning of the Eocene epoch lies a vast gap of at least several million years in duration. During the long ages in which the dinosaurs flourished the mammals remained small and inconspicuous and left as fossils tantalizingly few teeth and jaws, and hardly any skulls. A few years ago Mr. Barnum Brown of the American Museum of Natural History found embedded under the skull of one of the great dinosaurs of the Upper Cretaceous the fossil skull and jaw of a small mammal (fig. 1, VII) which has proved to be closely related to the existing opossum. Solely upon anatomical evidence it has long been recognized that the opossum is one of the most conservative types of mammalian living fossils now extant, and that in fact it has preserved most of the essential characters of a pre-placental Mesozoic mammal. Hence it is significant to observe that in the lateral aspect of the skull the Cretaceous and modern opossums retained a strong fundamental resemblance to the most progressive of the mammal-like reptiles.

In this seventh stage, however, the basal mammalian characteristics are well established. The most important fact is that the ascending ramus of the lower jaw has grown backward and upward until it has gained contact with the squamous portion of the temporal bone, where it

has formed a new joint, the temporomandibular articulation common to all mammals, but not yet achieved by any reptiles, with the doubtful exception of some of the diademodont division of the cynodonts. Meanwhile the lower jaw elements behind the dentary, namely the surangular, angular and articular, have disappeared, at least from the lateral aspect of the skull. To anyone who may doubt the validity of these inferences we may recommend the thorough study of the numerous papers of the late Professor E. Gaupp of Fribourg, one of the greatest morphologists of our time.

In passing from the reptilian to the primitive mammalian stage one sees clear examples of Williston's law of the progressive reduction of the skull elements. The supratemporal and intertemporal had long since disappeared. The cynodonts went further and dropped the postfrontal. The mammals went still further and cast off the prefrontal and the postorbital, leaving only the lacrymal and the jugal of the original five circumorbital bones.

But from the oldest mammals to man there will be no further reduction in the number of skull elements.

Light on the condition of the skull of the placental mammals during Cretaceous times has recently been gained from the highly important discovery by the Third Asiatic Expedition of the American Museum of six skulls and parts of skulls of Cretaceous mammals, which have recently been described by the author, in collaboration with Dr. G. G. Simpson of Yale University. These skulls lend much weight to the view of Huxley, Osborn, Weber and others that all the placental mammals of Eocene and later ages started from small insectivorous mammals. In spite of their far-reaching significance, I have not placed a figure of any of these skulls in the series showing ten structural stages for the reason that the exact arrangement of the sutural boundaries of the skull bones are for the most part not very clear. But in the form of the upper and lower premolars these Mongolian Cretaceous placentals afford an invaluable and long-sought-for stage in the evolution of the cheek teeth of the higher mammals, as I have shown in the December number of the American Journal of Physical Anthropology.

PRIMATES

Passing then to the eighth stage of the series, we come to the primitive Primates of the Eocene of North America, here represented by the genus *Notharctus* (fig. 1, VIII), excellent skulls of which are in my custody at the American Museum of Natural History. In the earlier mammals, as noted above, the postorbital bone had already disappeared, so that the temporal fossa was broadly continuous with the orbits. But the Primates, like certain other lines of mammals, notably the ruminant artiodactyls, soon found it advantageous to protect the eye by a postorbital bar and at the same time to brace the origin of the masseter muscle. Accordingly the frontal bone, which in the earlier vertebrates had been shut off from the orbits by the pre- and postfrontals, after the disappearance of these elements became the dominant element of the orbit and now sent downward a strong process, the postorbital process of the frontal, which met a similar process uprising from the jugal or malar bone. The eyes in the primitive stage are directed outward and forward. Meanwhile the angular process on the dentary or inferior maxillary had become prominent in correlation with the increasing obliquity of the pterygoid and masseter muscles. In the upper jaw the maxilla now effected contact with the frontal. The dental formula of *Notharctus*,

namely $I\frac{2}{2}$ $C\frac{1}{1}$ $Pm\frac{4}{4}$ $M\frac{3}{3}$ is undoubtedly more primitive than that of any higher primates.

The ninth stage of the present series is represented by the chimpanzee (fig. 1, IX), which is the least specialized in skull form of the existing great apes. From the detailed comparison of the jaws and teeth of the chimpanzee with those of the various species of fossil anthropoids referred to *Dryopithecus* and allied genera, I conclude that the skull form of female chimpanzees is not widely different from that of the inferred common ancestor of man and the higher anthropoids, and I find strong support for this view in many directions, especially in the masterly last work of that great anthropologist, Schwalbe.

Granting then for the moment at least, the view that in many respects the skull of a female chimpanzee is less advanced along the path of evolution than that of man, how does it differ from that of the vastly older and truly primitive skull of the Eocene primate *Notharctus*?

In the first place the chimpanzee has advanced beyond its Eocene predecessor in the forward shifting of the orbits, which are now directed completely forward so as to effect binocular, stereoscopic vision. Next, the postorbital septum now completely separates the orbit from the temporal fossa in the lateral view. With the assumption of brachiating habits the braincase has rotated forward and downward upon the column and the face has bent downward upon the basicranial axis. The premaxilla has fused with the superior maxilla and the greatest diameter of the latter is now approaching the vertical plane. Th maxilla sends forth two prongs on either side of the lacrymal, the former touching the frontal, the latter separating the lacrymal from the jugal. The dental formula has become reduced by the elimination of the first two premolars above and below, so that it is now the same as in man; the jaw is shortened and thickened, its angular process has also expanded and merges anteriorly with the mandible itself.

MAN

Viewed in this perspective the human skull (fig. 1, X) is seen to differ from that of the chimpanzee chiefly in the further development of the characters by which the chimpanzee differed from the earlier Primates. All the advances gained by the chimpanzee have in fact been carried much further in man. The greatest diameter of the face is now vertical rather than horizontal and the shortened jaws have been retracted beneath the forwardly-swelling and now enormous braincase. The dentition is enfeebled and a chin is present. But all these and many other differences are quantitative, not qualitative, and they measure the extent to which the human skull has outdistanced that of its humble relative, the chimpanzee.

CHANGES IN THE BONES OF THE SKULL

To recapitulate, the outstanding changes in the lateral view of the skull from fish to man appear to have been as follows:

Of the bones on the roof of the skull (fig. 2), namely the nasals, frontals, parietals, interparietals (or dermo-supraoccipitals) and tabulars, only the last disappear entirely in the mammals. As the brain enlarges these roofing bones are lifted into greater prominence, the frontals, parietals, interparietals and occipitals becoming the dominant elements in the great vault of the human skull.

The superior maxillary bone (fig. 3) begins as a slender, vertically shallow element, but by the time of the early

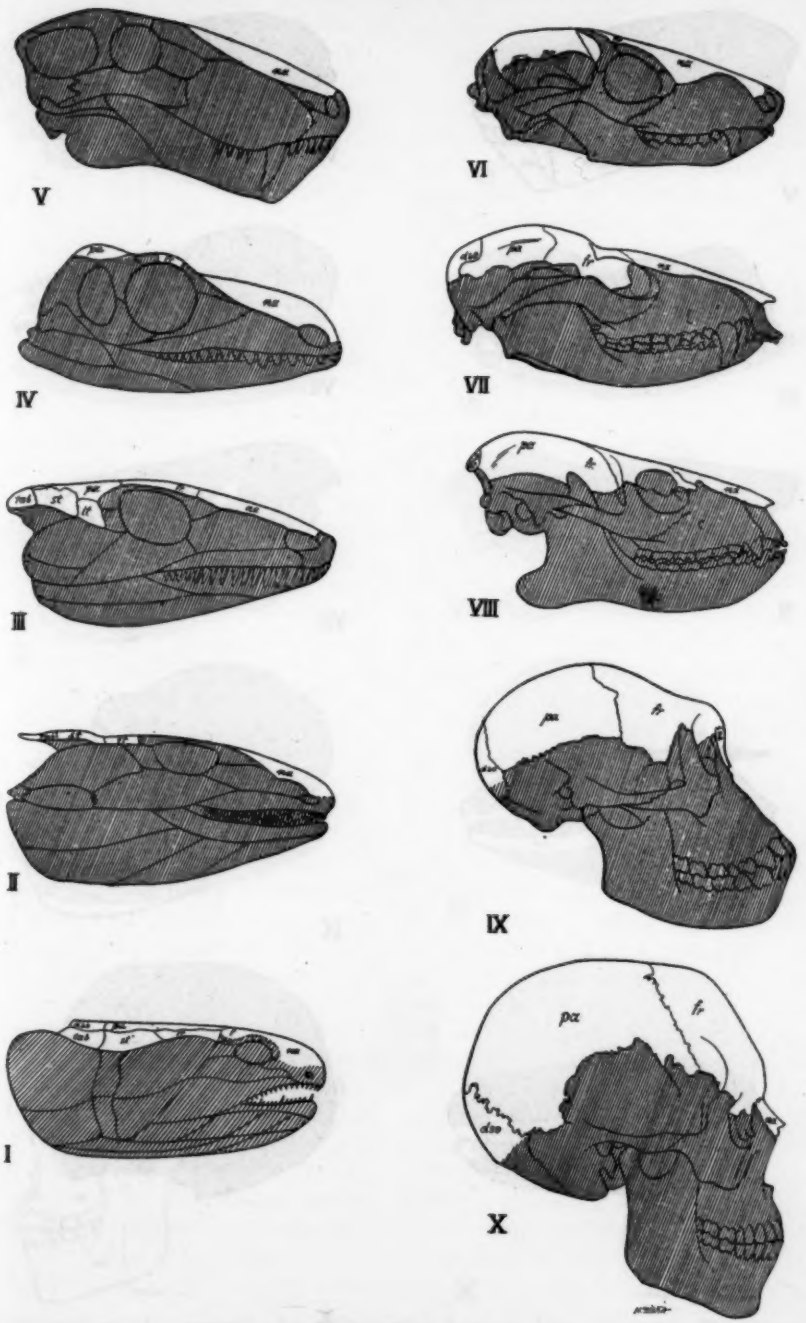


FIG. 2. EVOLUTION OF THE ROOF BONES

Stages as in fig. 1, except VII, in which the modern instead of the Cretaceous opossum is used

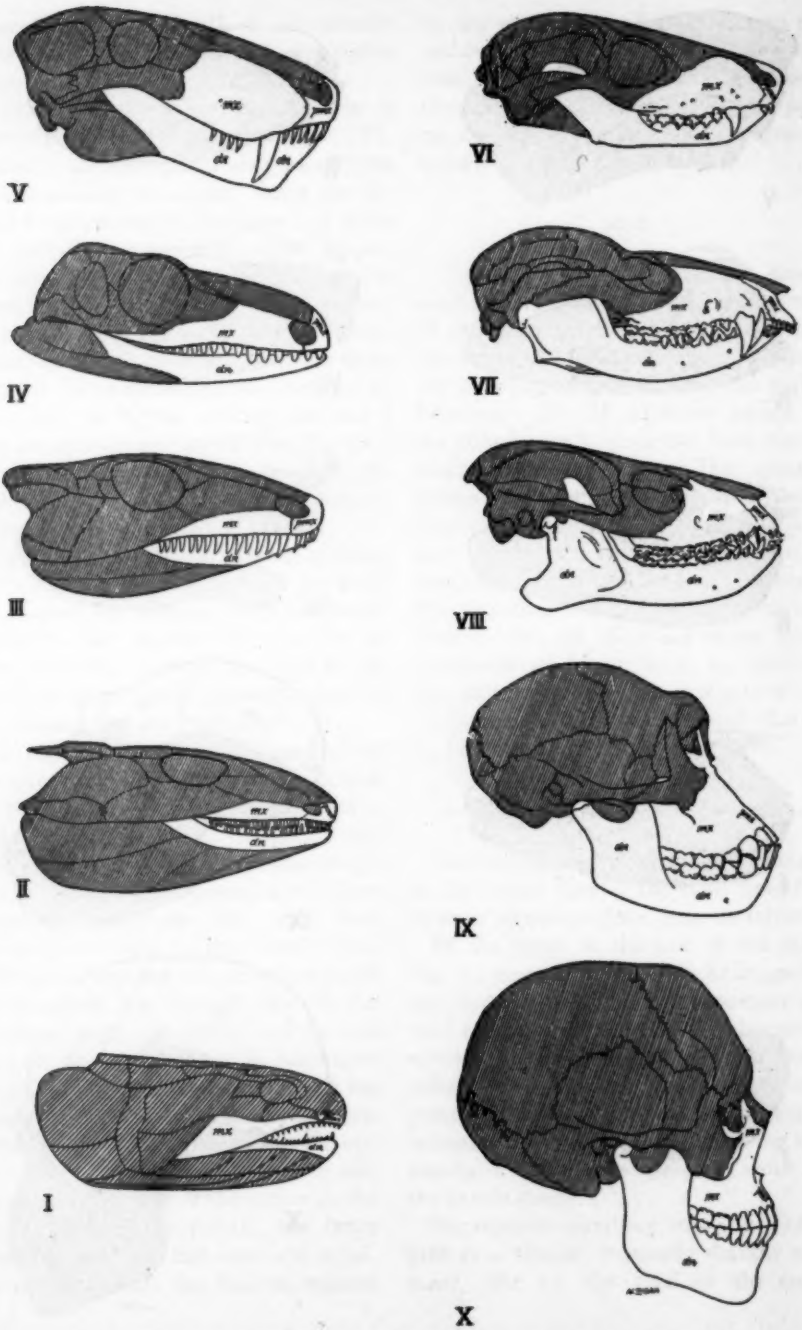


FIG. 3. EVOLUTION OF THE MAXILLARY SERIES (PREMAXILLA, MAXILLA, DENTARY)

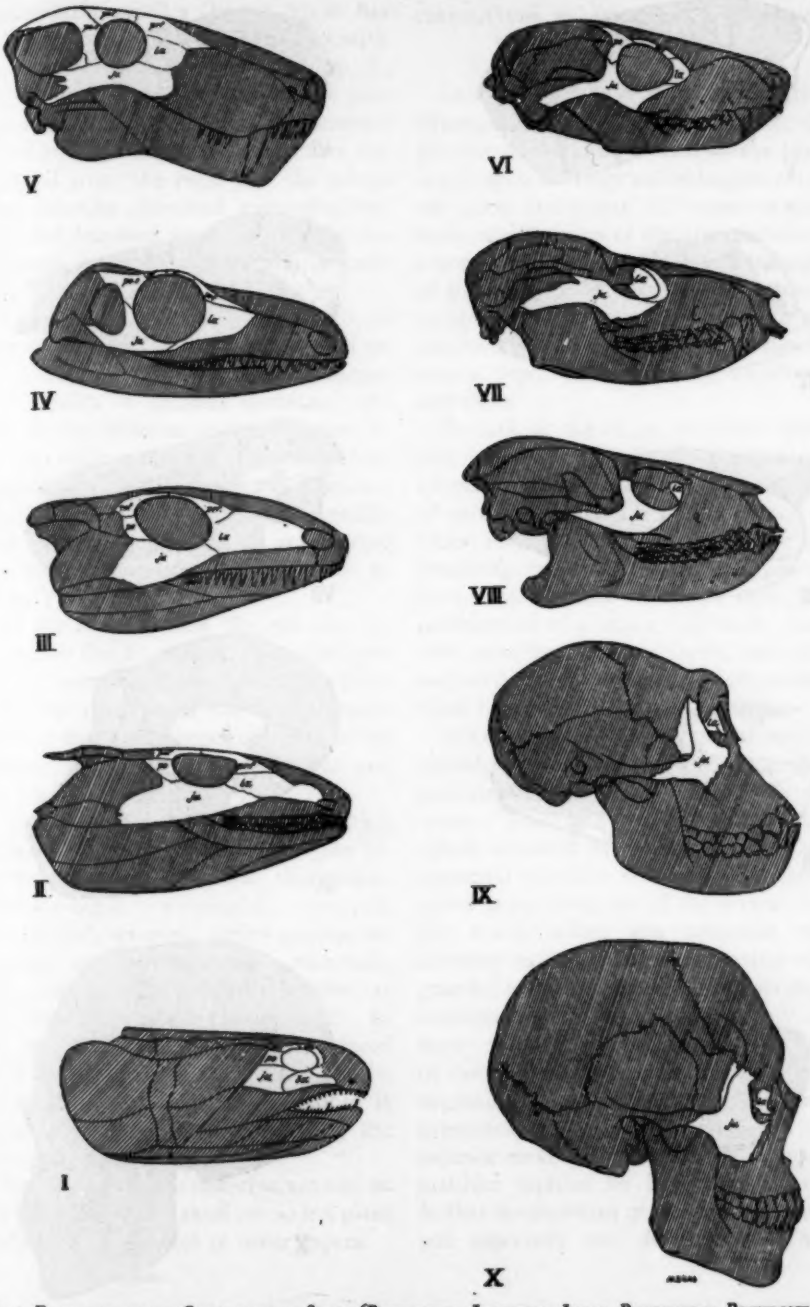


FIG. 4. EVOLUTION OF THE CIRCUMORBITAL SERIES (PREFRONTAL, LACRYMAL, JUGAL, POSTORBITAL, POSTFRONTAL)

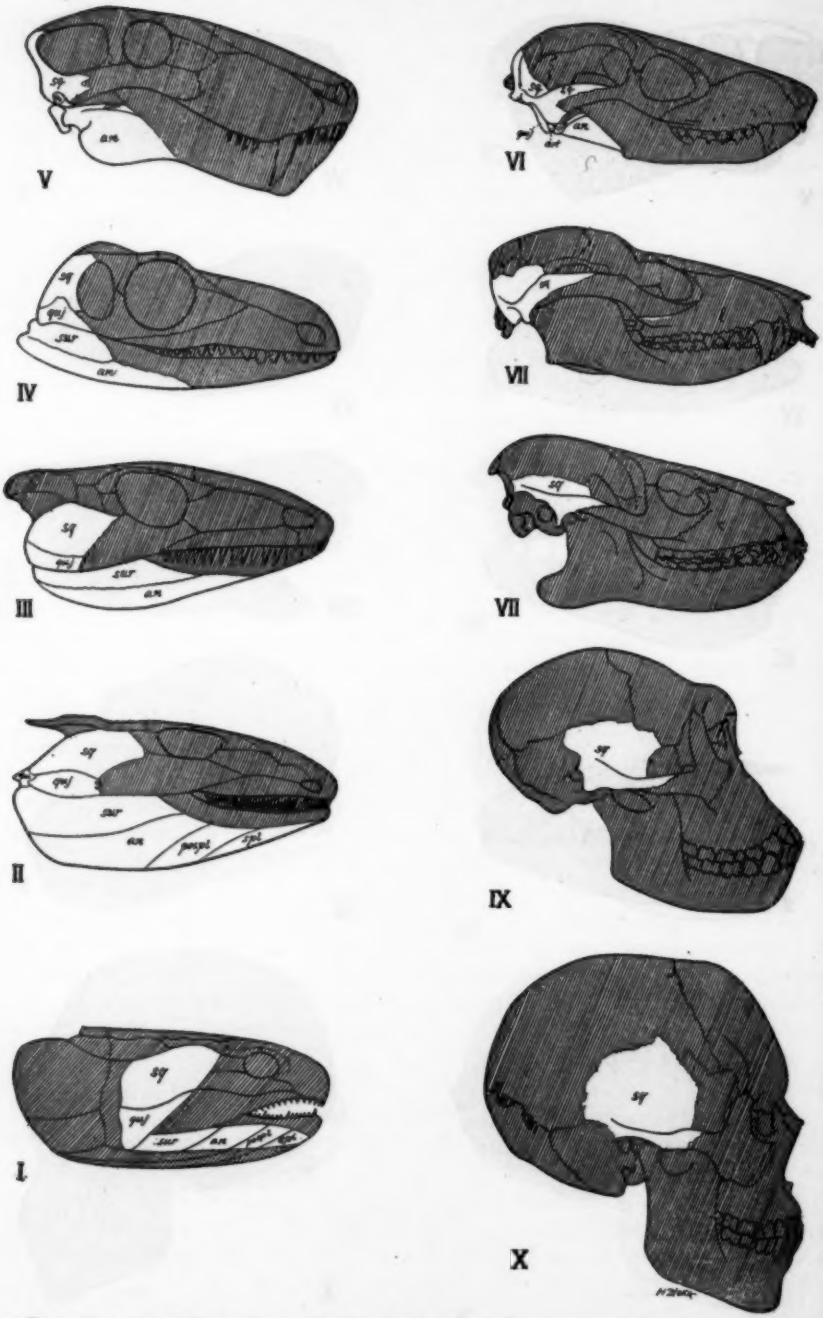


FIG. 5. EVOLUTION OF THE TEMPORO-MANDIBULAR SERIES (SQUAMOSAL, QUADRATO-JUGAL, SURANGULAR, ANGULAR, POSTSPLENIAL, SPLENIAL)

mammal-like reptiles (fig. 3, V) it has extended dorsally and gained contact with the nasals. In the mammals (fig. 3, VII–X) its dominance is still more pronounced; one fork reaches the frontals while another fork finally separates the lacrymal from the jugal and the whole bone becomes shortened antero-horizontally and deepened vertically. In the anthropoids and man the premaxillæ early unite with the maxillæ.

The inferior maxillary (dentary) at first is confined to the anterior half of the mandible. In the higher mammal-like reptiles it becomes dominant, the post-dentary elements retreating before it. In the earliest mammals the ascending ramus of the dentary effects a new contact with the squamosal, the temporo-mandibular articulation, which is transmitted without further essential modification to man.

Of the bones around the eye (fig. 4), originally five in number, three (the prefrontal, postfrontal, postorbital) are eliminated by the time of the earliest mammals, so that man inherits only two of the original five, namely the lacrymal and the jugal or malar.

The temporo-mandibular series (fig. 5), originally including eight bones (the intertemporal, supratemporal, squamosal, quadrato-jugal, surangular, angular, postsplenial, splenial), suffers gradual reduction, until in the earliest mammals, as in man, only the squamosal remains, at least in the lateral view of the skull. In the mammals the squamosal has fused with the enlarged periotic mass and in the anthropoids and man the tympanic is added, the whole complex forming the temporal bone.

The changes in the endocranium and on the under side of the skull are no less plain and will be dealt with in other papers.

CORRELATION OF STRUCTURAL EVOLUTION AND CHANGES IN HABITS

At every successive stage of evolution advances in skull structure were dependent upon improvements in the brain itself, upon shiftings and enlargements of the parts containing the sense organs, upon modifications of the jaws and teeth, accompanying or accompanied by changes of habits. The skull in turn is closely integrated with both the active and the passive elements of the locomotor apparatus, a topic which will be developed elsewhere.

To each of the stages described above man owes certain "basic patents," or adaptive improvements which have been of critical importance in his survival. Thus to certain far-off Devonian air-breathing fishes man owes the general ground plan of the vertebrate skull, the combination of primary "gill-arch" jaws with sheathing or outer jaws, and each and every one of the twenty-eight normal skull bones which he still retains.

Next, he is indebted to the first amphibians for partially solving the innumerable problems caused by emergence from the water. These old pioneers cast off the whole series of bones that covered the branchial chamber and made for themselves an ear drum out of the skin around the notch where the opercular was formerly located. The early reptiles safeguarded most of the inheritance from their semi-aquatic ancestors, dropping only the inter- and supratemporals. To the first of the mammal-like series man owes the beginnings of his temporal fossa and zygomatic arch, and the dominance of the superior maxilla. From the higher mammal-like reptiles he has inherited the further development of the temporal fossa and especially the dominance of the

inferior maxillary or dentary bone of the lower jaw. To these progressive pro-mammals man can render thanks for the differentiation of his dentition into incisors, canines, premolars and molars, and apparently he can also thank them for the reduction of the numerous successional teeth to two sets, corresponding to the milk teeth and the permanent set.

The earliest mammals invented one of the most useful features of man's skull by eliminating from the masticatory apparatus all the elements lying behind the dentary and by establishing the temporomandibular joint. They also cast off the reptilian prefrontal, postfrontal and postorbital bones and cleared the way for the final simplification of the bony scaffolding of the face.

To the earliest Primates, well schooled in arboreal life, man owes the first steps in the glorification of the eyes, which become increasingly dominant. These still lowly but thrifty forebears made good the loss of the reptilian postorbital bar by elaborating a new one from conjoining processes from the frontal and jugal (or malar) bones.

But still greater was our debt to the arboreal pro-anthropoids, those intelligent beings who elected to develop sight at the expense of smell. These skilled acrobats, moving in a vertical position, met and solved a new series of problems connected with the turning downward of the skull upon the upright column. They also made the first notable attempts to shorten and deepen the face and even took a long step toward enlarging the brain and brain chamber.

Starting with these and many like advantages gained during a long training in arboreal life, it was the task of our relatively nearer precursors (beginning possibly in Miocene times, or earlier) to re-adapt all these arboreal adaptations for a life on the ground and to take the final steps upward that have brought humanity to its present relatively high level of intelligence.

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NEW BIOLOGICAL BOOKS

The aim of this department is to give the reader brief indications of the character, the content, and the value of new books in the various fields of biology. In addition there will usually appear in each number one longer critical review of a book of special significance. Authors and publishers of biological books should bear in mind that THE QUARTERLY REVIEW OF BIOLOGY *can notice in this department only such books as come to the office of the editor. The absence of a book, therefore, from the following and subsequent lists only means that we have not received it. All material for notice in this department should be addressed to Dr. Raymond Pearl, Editor of* THE QUARTERLY REVIEW OF BIOLOGY, *1901 East Madison Street, Baltimore, Maryland, U. S. A.*

A PHYSIOLOGICAL THEORY OF HEREDITY

Being a review of *Physiologische Theorie der Vererbung*, by Richard Goldschmidt. Berlin (Julius Springer), 1927. 4½ x 7; vi + 247, 59 figs. Reichsmark 15.

By W. E. Castle, Harvard University

An attempt has been made in this book to extend to heredity in general an explanation previously formulated by the author for the peculiar phenomena of sex determination and sex expression in the gipsy moth. Goldschmidt's fruitful discoveries in this field are well known to biologists, and any generalizations which he may have deduced from them merit careful consideration.

The bisexual state common in all the higher animals, in order to function properly for the perpetuation of the species must be represented in two—and only two—very different kind of individuals, males and females. Each of these must produce a different kind of reproductive cell or gamete, the female producing eggs, the male sperm. The female usually possesses also apparatus for the reception and, if necessary, for the storage of sperm, as well as for egg-laying, or the bringing forth of living young. The male must have suitable organs for bringing the sperm which he produces into effective relation with the eggs, either within or without the body of the female.

Apart from these essential or primary sex differences, there are other secondary ones which permit of the ready recognition of the sexes, such as the beard and voice of man, and the plumage of birds. The entire complex of one sex or the other is normally inherited as a whole. If this does not happen, disastrous consequences follow, as in the crossing of different geographic races of the gipsy moth, *Lymantria*, studied by Goldschmidt. Here individuals are produced which show mixed or intermediate sexual states affecting in varying degrees both primary and secondary sex organs. Such individuals, called intersexes by Goldschmidt, are usually incapable of reproduction because they possess neither one nor the other of the two interlocking sets of sex mechanism necessary for normal reproduction.

There is a certain analogy between the inheritance of the alternative sex states, male and female, and the inheritance of alternative dominants and recessives in Mendelian inheritance. This was recog-

nized by Mendel himself, as shown by his posthumously published letters, and it has been the basis of fruitful subsequent study of sex inheritance.

But there is this difference between the two cases, that a race may contain, and usually does contain, only *one* of two or more Mendelian allelomorphs, whereas maleness and femaleness must both occur in the same interbreeding population, if sexual reproduction is to occur.

It is not certain, therefore, that everything which happens in sexual reproduction will find its counterpart in heredity in general.

Goldschmidt assumes, at the outset, that all inheritance is Mendelian, and that its operation is through genes contained exclusively in the chromosomes. In other words he accepts in full the basic conclusions of the Morgan school as to the mechanism of heredity, although he finds much to criticise in matters of detail. He seeks to go beyond the findings of the Morgan school and explain *how* the genes operate in producing their end results.

Goldschmidt takes the operation of the sex genes (in sex determination) as his point of departure. A quantitative relation of such genes normally determines whether maleness or femaleness shall be expressed in the zygote. Thus in the squash bug, in *Drosophila*, and in a great many other animals including man, one X determines maleness but 2X femaleness in all their varied forms of expression. There is no other point of organic equilibrium for the zygote except maleness or femaleness. These are expressed in their entirety, one or the other, according as a single or a double dose is present of the genes contained in the X-chromosome. The facts so far are incontestable. Goldschmidt goes further and assumes that there are specific genes for maleness (M) and femaleness (F). Here he is on less certain ground. He bases the assumption on the wide variety of characters in whose production genes are known to be involved. Sex is just one more case. But Riddle has shown in the case of pigeons and Lillie in cattle, that an embryo which would otherwise be a female may be changed into a male, or *vice versa*, without disturbing the gene apparatus, but merely by manipulation of other conditions, as for example the oxidation rate of the egg cytoplasm, or the chemical composition of the blood of the embryo. These findings are supported by a large amount of experimental evidence on other animals. Further Bridges has shown that *all* the chromosomes of *Drosophila* have a "net influence" on the determination of sex, directing the development of the embryo either toward maleness or toward femaleness. Now it is possible to assume that in these chromosomes there are specific genes for maleness and femaleness, but it is not possible to make this assumption for an influence wholly outside the chromosomes, such as an environmental condition. The assumption is therefore superfluous that specific genes for maleness and femaleness are involved in sex determination. It may be only that the embryo in development may according to circumstances assume one or the other of two alternative forms. The determining factors are partly internal, partly external. No one refers to the external factors as genes for maleness or femaleness; why need we assume that the internal ones are such?

It may be suggested that this line of argument is destructive of the entire theory of the gene, because many characters for which we postulate genes are influenced in their expression by external conditions also. By gene we mean a substance or molecule or material influence of some sort occupying a particular locus

in or on a chromosome and capable of influencing the development of some character in the organism. We hesitate to extend the theory of the gene to the organism-as-a-whole, lest it cease to have utility. Who has assumed the existence of genes for vertebrateness and genes for arthropodness? Perhaps if we could cross the two groups, some one would have the hardihood to postulate a multiple factor system for each. The current explanation of blending inheritance is scarcely less fanciful. But until such cases as phylum differences have been analyzed into genes, we may properly object to the assumption of specific genes for maleness and femaleness. Nevertheless we must allow Goldschmidt to make the assumption in order to formulate his theory, but like the attorney for the defense we register an objection with the court.

Goldschmidt's formulation proceeds as follows:

1. Each sex contains the genes for the differentiation of both sexes.

2. Of these genes one sort is in the X-chromosome (F in *Drosophila*, M in *Lymantria*), so that in one sex it is present in a double dose, in the other in single dose.

3. The other sort of sex gene (M in *Drosophila*, F in *Lymantria*) is present in the autosomes of *Drosophila*, and in the Y-chromosome of *Lymantria*. In *Lymantria* modifying factors of sex are assumed to be present in the autosomes also.

4. The relative quantity of the sex genes determines the sex of the embryo, the operation of each sort of gene being proportional to its quantity.

5. Normally the dosage of genes must be such that the quantity of the sort lying outside the X-chromosomes shall be greater than that of the sort lying in *one* X-chromosome but less than that lying in *two* X-chromosomes.

6. Proportional to the quantity of each kind of sex gene is assumed to be the rapidity of the corresponding sex determining reaction.

7. Male and female sex determining reactions occur simultaneously but the one which progresses more rapidly controls differentiation.

8. The reactions consist in the production of substances controlling sexual differentiation, which progresses normally only in one of two possible (male or female) directions.

Goldschmidt regards points 1, 2, 3, and 5 as of especial importance in relation to the question of sex determination, the others as the basis for his general theory of heredity. Point 1 he regards as beyond doubt. If one concedes the assumption that there are specific genes for maleness and femaleness, then it must be granted further in view of the experimental evidence that every individual contains genes of both sorts, that is has at the outset the capacity to develop either into a male or into a female. It is inconceivable to him that there should be wholly sexless individuals. But such individuals should be conceivable if maleness and femaleness are produced by specific genes. Mutation in or loss of these genes should be capable of producing either individuals which lack the genes for one sex, or those which have no sex genes whatever. The fact that such individuals are unknown argues against the fundamental assumption that specific sex genes exist.

Points 4 and 5 are deduced by Goldschmidt from the important results of his crosses between different geographic races of the gipsy moth, *Lymantria*. By certain of these crosses he has shown that individuals are regularly produced which in their sexual characters are intermediate between males and females, and which he calls "intersexes."

The normal result of intra-racial matings is the production of equal numbers of males and females. When intersexes are produced, they occur as a modification of one or the other of the normal sex groups, but not both. Accordingly Goldschmidt designates them *female intersexes* when they occur in association with normal males and *male intersexes* when they occur in association with normal females.

Races may be grouped, as regards their genetic properties, into the strong, the neutral, and the weak. A female of a weak race crossed with a male of a strong race produces female intersexes, whereas the result of the reciprocal cross is normal in F_1, but includes male intersexes in F_2. Neutral races give a normal result when crossed with any other race. A normal result is also produced by crossing weak with weak or strong with strong. It is only crosses of weak with strong that produce intersexes. There are different degrees of intersexuality forming a complete transition from male to female, or *vice versa*, and ending with the complete transformation of what is expected to be one sex into the other. Different strong or weak races produce different degrees of intersexuality in crosses. Thus a female of weak race *A*, with a male of strong race *M*, produces along with normal F_1 males, females only slightly intersexual (that is showing male-like characters). But a female of the same weak race *A* mated to a male of strong race *N*, produces females moderately intersexual (male-like), and if mated with a male of strong race *O* produces only strongly intersexual females (in addition to the usual 50 per cent of normal males). Thus among strong and weak races there are different degrees of strength and weakness.

This series of facts is regarded by Goldschmidt as showing not only that there are separate specific genes for maleness and femaleness but that these vary quantitatively from race to race and on the basis of these quantitative variations accurate predictions can be made as to the results of particular matings.

Similar conclusions are drawn from a review of Bridges' experiments with triploid and partially triploid individuals of *Drosophila*. Bridges showed that triploidy or tetraploidy affecting all chromosomes alike does not alter the sex, but if the autosomes become triploid while X remains diploid an intersex results (a female showing male characters). On the other hand if X becomes triploid while the autosomes are diploid, a "superfemale" results. That is X has a female producing tendency, and the autosomes a male producing tendency. What the zygote becomes, depends on the proportions in which the different kinds of chromosomes are present. The later results of Bridges indicate that chromosome IV as well as X has a net female producing tendency, leaving the net male producing tendency to chromosomes II and III only.

It is undeniable, in view of the very positive results of both Goldschmidt and Bridges, that chromosomes as wholes do have a very positive influence in sex determination. The X in *Drosophila* has a female determining influence, that of *Lymantria* according to Goldschmidt is male determining. With racial variation, which undoubtedly involves changes in chromosome constitution, the sex-determining influence of chromosomes varies. Whether there are particular parts of chromosomes or special genes which exert an influence toward male or female differentiation remains to be demonstrated.

Goldschmidt explains the varying degrees of intersexuality observed in his cultures in the following way. He observes that in individuals only slightly

intersexual the organs affected are only those latest to be differentiated. As the degree of intersexuality increases, additional organs are affected which were earlier differentiated, until in extreme cases even the earliest differentiated organs become affected and a complete sex transformation results. He concludes that an intersex is an individual which up to a certain point develops as of one sex (as determined by the ordinary X-Y chromosome apparatus) but beyond that point develops as an individual of the *opposite* sex. As to the *fact* of an abrupt change in sex character during the process of development he is very positive. The workers on *Drosophila*, so far as I know, have not observed this, but Crew thinks something of the kind occurs in amphibian and mammalian intersexes. Why does a change in sex differentiation occur during the development of intersexes? Goldschmidt's explanation is as follows: 1. The quantitative relation of the male and female sex genes determines which at the outset shall be operative. The greater controls. Thus a genetic male produces more *male stuff* than female stuff, and a genetic female produces more *female stuff* than male stuff. The kind of stuff which is in excess controls differentiation. Ordinarily the initial excess persists throughout development but in an intersex it is replaced at some point by an excess of the opposite sort of stuff which then takes control. 2. The sex gene which is greater at the outset produces sex stuff more rapidly and so holds control so long as it has suitable material to act upon. But it may use up this material sooner because of the very energy of its initial enzymatic activity and so its product may later fall off, whereas the originally weaker gene acting with more moderation may later have a greater total output and so take control.

This basic conception, if I have rightly understood it, is elaborated by Goldschmidt at great length and with endless diagrams which serve rather to confuse than to clarify the situation in the mind of the ordinary reader. Such a reader would like to see the alleged fact substantiated that in the development of an intersex there is a definite change during ontogeny from femaleness to maleness or *vice versa*, and not a balanced relation of maleness and femaleness *throughout development*, as has been commonly supposed. Goldschmidt does not think it necessary in this publication to submit evidence in support of his statement that such a change in sex occurs. He refers the reader to earlier and more detailed publications.

The greater part of his book is devoted to the extension to heredity in general of the explanation formulated for sex inheritance. The phenomena of Mendelian inheritance including dominance are regarded as consequences of 1) differences in quantitative value of genes, 2) in consequent rates of reaction, and 3) production of formative stuffs, limited by 4) the catalytic nature of genes in general.

A gene is considered to be a material particle of a particular quality present at the beginning of development in definite quantity in a particular chromosome. It begins its action to produce a formative stuff as soon as development begins, but this will not become effective until it has the proper quantitative relation to other competing formative stuffs. Thus a recessive gene has no visible effect so long as the corresponding dominant is present along with it. The genes on Goldschmidt's view are catalytic substances—sometimes he suggests that they are autocatalytic—, the formative stuffs which they produce are hormone-like in action.

It is difficult for the ordinary student of

genetics to estimate the value of this theory which involves many unproved assumptions as to the chemical events occurring in ontogeny. To the ordinary geneticist a gene in the guise of an autocatalytic enzyme is as incomprehensible and much less usable than when designated simply *A* or *a*. Only an expert biochemist working at the elbow of a geneticist will be able to say whether Goldschmidt's theory is a step forward in the explanation of heredity, or pure bunkum. But Goldschmidt's genetic studies of *Lymantria* have brought to light many important and significant facts. Possibly a simpler explanation than his may be found for some of them, but at any rate they merit careful attention and verification, if possible, on other organisms.

BRIEF NOTICES

EVOLUTION

HOLISM AND EVOLUTION.

By J. C. Smuts. *The Macmillan Co.*
$3.50 8½ x 5½; vii + 362. *New York*

This is a remarkable book. There are few persons in the history of the world who, having pursued throughout their adult life an active political career and achieved in it such success as is implied by a premiership, are either inclined or able then to make a first-rate contribution to the philosophy of science. And precisely this is what General Smuts has done. *Holism and Evolution* will stand comparison on its merits with anything that has yet been written on emergent evolution.

The viewpoint and scope of the book are indicated by the following quotation:

> This work deals with some of the problems which fall within the debatable borderland between Science and Philosophy. It is a book neither of Science nor of Philosophy, but of some points of contact between the two. To my mind it is the surface of contact between the two that will prove fruitful and creative for future progress in both, and to which special attention should be directed. Some border problems between the two are here considered in the light of recent advances in physical and biological science. And a re-examination of fundamental concepts in the light of these advances reveals the existence of a hitherto neglected factor or principle of a very important character. This factor, called Holism in the sequel, underlies the synthetic tendency in the universe, and is the principle which makes for the origin and progress of wholes in the universe. An attempt is made to show that this whole-making or holistic tendency is fundamental in nature, that it has a well-marked ascertainable character, and that Evolution is nothing but the gradual development and stratification of progressive series of wholes, stretching from the inorganic beginnings to the highest levels of spiritual creation. This work deals with our primary concepts of matter, life, mind, and personality in the light of this principle, and discusses some of the problems of Evolution from this new point of view.

The remarkable things about the book are first its originality and freshness of viewpoint, and second the extraordinary familiarity which the author displays with the trends of inquiry and results in various fields of science. To be sure a good deal of General Smuts' reading of science has obviously been from second rather than first hand sources. But on the whole he has kept his balance very successfully. We urge all biologists to read the book, whether they agree with its viewpoint and conclusions, or do not.

EVOLUTION NOT IRRELIGIOUS. *Some General Aspects of Evolution and Relations to Religion.*

By Walter C. Kraatz. *Walter C. Kraatz*
20 cents 5 x 7; 28 (paper) *Akron, Ohio*

A "reconciliation" document by the

assistant professor of biology in the University of Akron, Ohio. That man was descended from a monkey is branded as a "common misconception." But just what he was descended from is not made clear. However, "man's evolution is demonstrated."

A static mind must have a static idea to cherish and fight for. The others being taken from under them so completely by the more mathematically demonstrable sciences, they concentrate their attacks upon organic evolution, or especially that part, evolution of man, where they know that their emotional appeals may strike a popular chord. Man is unavoidably somewhat egotistic and proud of his high degree. Like the 'new rich' in human society he often disdains his poor beginnings. This policy is not commendable, not Christian-like. It is prejudiced and entirely unscientific. The scientific attitude must be secured if the consideration of man is going to be made as fairly and as judiciously as the consideration of the rest of the world. The scientific attitude and method are the same in all the sciences: But since biology is not a mathematical science like astronomy we must presumably for a few more centuries bear with the semi-occasional attacks of the opposition.

Magna est veritas!

MAN CREATED DURING DESCENT *at the beginning of the New Stone Age, that is, not more than about five or seven dozen centuries ago.*

By Rev. Morris Morris.

Marshall Brothers, Ltd.

3s. 6d. 7¼ x 5; 111 *London*

Here comes a new fundamentalist, much higher-toned than Mr. Bryan and his like. Mr. Morris was once a research scholar in geology in the University of Melbourne. He has all the technique of scholarship and documents his case from separately unimpeachable scientific sources, but some of the authors quoted will be surprised to see the conclusions to which their innocent and carefully safeguarded statements lead, when skilfully marshalled by Mr. Morris to support his thesis. The general idea of this thesis is that when business is dull, or as the occasion seems to require it, some special creating is done. In the intervals evolution by natural causes goes on. The last time any considerable job was done in the creating way was at the end of the Old Stone Age. Man was then made, more or less just like that! The proof is found in the relics of the neolithic culture. They were made by *men*. Mr. Morris says so. The paleolithic artefacts were the products of "individuals of a lower species," erroneously called "man" by archeologists and anthropologists. The book is an interesting example of how a shrewd dialectician can use sound data to establish a ridiculous conclusion.

SCIENCE AND ULTIMATE TRUTH.

Fison Memorial Lecture, 1926. Delivered at Guy's Hospital Medical School, March 25, 1926.

Rev. W. R. Inge. Longmans, Green and Co.

65 cents 5½ x 8½; 32 *New York*

Anything that the distinguished Dean of St. Paul's writes is sure to be interesting, stimulating, and clear-headed to a degree not commonly attained in the lucubrations of the reverend clergy. This lecture is a contribution to the literature of reconciliation. But it has a higher and different tone than the efforts in the same direction of some of our distinguished scientific colleagues in this country, as the following passage sufficiently shows: "I know that religion, science, and art are all jealous of each other, because each of them claims, in a sense, to cover the whole field, that is, to interpret all experience from its own point of view. Philosophy tries to mediate between them,

and the task has so far been beyond its powers. Some of the new philosophies undermine the authority of science, as some of the older systems undermined the authority of religion. Complete reconciliation is not in sight; but there is no reason for hostility, which reacts unfavourably upon our whole view of life. For the eternal Values are not entirely separate from each other."

NOMOGENESIS *or Evolution Determined by Law.*

By Leo S. Berg. Introduction by D'Arcy W. Thompson. Translated from the Russian by J. N. Rostovtsow. *Constable and Co.*
28 shillings *London*
8½ x 5½; xviii + 477

This is orthogenesis all over again. To the author "evolution is in a great measure an unfolding of existing rudiments." The "fundamental property of the living 'being' is purposive structure." But how the "purposive structure" came to be so is not explained. Berg takes it as axiomatic, and deduces from it support for Lamarck and rejection of Darwin. Philosophically the book is not very penetrating. Biologically it contains a lot of interesting observations and discussions. It has a bibliography of 20-odd pages, and a detailed index.

SELECTED ARTICLES ON EVOLUTION.

Compiled by Edith M. Phelps.
The H. W. Wilson Co.
$2.40 8 x 5½; liii + 283 *New York*

This book is neutral. Biologists and fundamentalists are each allowed their say, in the form of quotations. It is essentially a book of the "debater manual" type, from which the superficial and hurried student can get "something on both sides." To the biologist it will chiefly be useful in collecting in one volume some choice fundamentalist tidbits where he can find them easily. The editing is well done.

AUTHORITATIVE STATEMENTS ON SCIENCE, EVOLUTION, RELIGION AND THE BIBLE.

Compiled by Samuel S. Wyer. *S. S. Wyer.*
1014 Hartman Bldg., Columbus, O.
9 x 6; 31 (paper)

Another reconciliation document by a consulting engineer, consisting of some 25 pages of short quotations from 41 bibliographically cited authorities, with Lyman Abbott heading the list and Albert E. Wiggam ending it. There is a laudatory introduction by President W. O. Thompson of Ohio State University. It is entertaining to see science overtly espousing authoritarianism as its ultimate controversial method. How the Jesuit theologians must be chuckling!

EVOLUTION.

By J. Graham Kerr. *The Macmillan Co.*
$3.50 9 x 6; x + 278 *New York*

An elementary treatise on organic evolution for the beginner in science and the general reader. The author is Regius Professor of Zoology at Glasgow. The treatment of the old material and arguments is carried out in a refreshingly vigorous and unhackneyed way, in part because emphasis is placed throughout on the importance of old-fashioned field natural history as the groundwork of knowledge of evolution.

GENETICS

CONTRIBUTIONS TO A KNOWLEDGE OF INHERITANCE IN MAMMALS. *I.*

Studies of Color Inheritance and of Linkage in Rabbits, by W. E. Castle. II. Fertility and Sterility in the Norway Rat, Mus norvegicus, by Horace W. Feldman. III. The Japanese Waltzing Mouse: Its Origin, Heredity and Relation to the Genetic Characters of Other Varieties of Mice, by William H. Gates. (Publication No. 337.)

Carnegie Institution of Washington
$1.50 *Washington, D. C.*
6¾ x 10; 138 + 14 plates (paper)

All three of these papers are important contributions to genetics. Castle concludes that in rabbits "the factors Dutch, English and Angora are linked; their genes lie in one chromosome, probably in the order named. Only 0.1 per cent of crossing-over has been observed between Dutch and English but each of these genes shows crossing-over with Angora in a frequency of 12 to 14 per cent. The only other positively demonstrated linkage in rabbits is between the color-factor and its allelomorphs on one hand and black *vs.* brown on the other hand. Both these genes lie in the same chromosome and the cross-over percentage between them is 34.6 ± 1.0. No positive evidence has been obtained of a difference in rate of crossing-over between the two sexes in rabbits, though such a difference is known to occur in rats and mice. The nine unifactorial characters of rabbits discussed in this paper, as indicated by their linkage relations, lie in 6 different chromosome pairs, only 2 of which contain more than one known gene."

Gates' interesting study of the Japanese waltzing mouse leads to the following conclusions: "That the pure race of Japanese waltzing mice has been derived from the wild form, *Mus wagneri*, and that the varietal name of *Mus wagneri rotans* Fortuyn is therefore valid. That in interspecific crosses of animals (mice) as in plants, there is a tendency for each set of parental chromosomes to group together, forming associate systems which may materially modify Mendelian ratios. That interspecific hybrids in mice exhibit heterosis to a marked degree as to size, vitality, longevity, and metabolic and reproductive activities. That the factor for waltzing is neither sex-linked nor linked with the factors for agouti, albinism, pink-eye, dilution, brown, Dutch spotting, short-ear or kinky-tail. Waltzing shows no evidence of association with any lethal factor. That the character, kinky-tail, is definitely linked, either genetically or physiologically, with short-ear."

THE PHYSIOLOGY OF THE CONTINUITY OF LIFE.

By D. Noël Paton. *The Macmillan Co.*
$4.00 8½ x 5½; x + 226 *New York*

This is a poor book. The author, who is a human physiologist, takes as his motto *Jurare in verbis nullius magistri*. This is a noble and altogether praiseworthy sentiment. But the author makes it the excuse to dismiss as worthless or dubious about all of the most solidly established facts and ideas of modern genetics, and to accept with laudation a good part of the most arrant quackery and pseudo-science which biology has achieved in the last quarter-century. Much of the book deals with material with which the author has no first hand familiarity. The result is sad. And in detail the writing is carelessly done. The distinguished Dutch botanist masquerades under the cognomen "Von Vries." Only the context enables the informed reader to recognize that by "Rotier" is meant rotifer. The book is a

fine example of the folly of a specialist in one field of science trying to expound critically the highly technical results of another.

HEREDITY. *Fifth Edition, Thoroughly Revised.*
By J. Arthur Thomson. *R. V. Coleman*
$5.00 (*522 Fifth Ave.*) *N. Y.*
8¼ x 5½; xiv + 542

To what was in former editions this revision adds some of the newer results in the field of genetics. But the general point of view seems, on the whole, a bit old-fashioned. Perhaps it is a good thing to have such a book available, in which a wise, mature and philosophical general biologist, who is not a specialist in genetics, reviews in a detached way the whole field. But he does incorporate some dreadful bunk from the literature, without any indication that his critical blood-pressure has been elevated in the least.

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Lieferung 204.* Containing following articles: *Züchtung der Homopteren*, by Carl Börner; *Züchtung von Neuropteren, Trichopteren und Panorpaten (Mecopteren)*, by Franz Heikertinger; *Züchtung der Lepidopteren*, by E. Fischer; *Züchtung von Dipteren*, and *Züchtung von Coleopteren*, by Franz Heikertinger; *Züchtung von Hymenopteren*, by Josef Fahringer.
Urban und Schwarzenberg
12.30 marks 7 x 10; 270 (paper) *Berlin*

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Lieferung 207.* Containing following articles: *Züchtung von Ameisen*, by Heinrich Kutter; *Haltung und Zucht der Honigbiene*, by Max Hartmann; *Die Zucht des chinesischen Seidenspinners (Bombyx mori)*, by Herbert Michael; *Methoden zum Züchten von Landpulmonaten*, by Karl Künkel; *Pflege und Zucht weiterer wirbelloser Landtiere*, by Paul Kammerer; *Methoden der biologischen Bekämpfung schädlicher Insekten im Pflanzenschutz*, by F. Stellwaag. *Urban und Schwarzenberg*
8.40 marks 7 x 10; 486 (paper) *Berlin*

These two numbers of the Abderhalden handbook are of particular interest to geneticists, giving practical directions for cultural procedures.

GENERAL BIOLOGY

ON THE TRAIL OF ANCIENT MAN. *A Narrative of the Field Work of the Central Asiatic Expeditions.*
By Roy Chapman Andrews (with an introduction and chapter by Henry Fairfield Osborn).
G. P. Putnam's Sons
$6.00 6¼ x 9¼; xxiv + 375 *New York*

The real romance of biology is in the field and not the laboratory. The achievements of the field naturalist are usually not so spectacular as those of the laboratory worker, though dinosaur eggs do produce a thrill if earnestly thought about. But the explorer does have a lot of fun along with and out of his proper work. This beautifully printed and illustrated volume gives a popular account of the Central Asiatic Expeditions of the American Museum of Natural History. Roy Chapman Andrews is a superb story-teller, as well as organizer and administrator. The person who does not get a thrill out of this book should consult a physician. Something is surely wrong with him. The title is a little misleading. So far the Expeditions have had no great luck with ancient man. But they are on the trail and the by-products are sufficiently exciting and important to justify more than one book.

DIE ANORGANISCHEN GRENZGEBIETE DER BIOLOGIE (*insbesondere der Kristallvergleich*).
By Hans Przibram. *Gebrüder Borntraeger*
7.50 marks 8¾ x 5½; 240 *Berlin*

An interesting review of the literature regarding the behavior of crystals, particularly in respect to growth and regeneration after removal of parts. The discussion is from the point of view of a biologist, who has himself done a good deal of original research in these fields of crystallography. The result is a serious and significant evaluation of the analogies between inorganic and organic behavior. There are 65 illustrations, a bibliography covering 23 pages, and a detailed index.

CUVIER ET LA SCIENCE DE LA NATURE.
By Louis Roule. *Ernest Flammarion*
9 francs 4¾ x 7¼; 246 (paper) *Paris*

This portion of the author's interesting attempt to write the history of biology around the lives of the great French naturalists is a useful and well-written contribution to the literature of the history of science. It is divided into three parts, of which the first is biographical; the second an analysis of Cuvier's writings; and the third an evaluation of the influence of Cuvier upon the current of biological thought, especially with reference to organic evolution. The author is professor at the Museum of Natural History.

BIOLOGY. *"Science for All" Series.*
By O. H. Latter. *John Murray*
3s. 6d. 7¼ x 5; vii + 197 *London*

Mr. Latter is well known as a successful teacher of biology. This little elementary text of general biology, with simple laboratory directions following each chapter, will be found by American teachers to contain many suggestive ideas likely to be useful in their work.

PROTOPLASMA. *Internationale Zeitschrift für Physikalische Chemie des Protoplasten. Band 1, Heft 1.* *Gebrüder Borntraeger*
14 marks *Berlin*
7¼ x 10¼; iv + 176 (paper)

Specialization and differentiation in the biological disciplines grows ever finer. THE QUARTERLY REVIEW OF BIOLOGY heartily welcomes this new-comer to the widening family of special journals. It is to concern itself with the physiology of protoplasm, looked at as a physico-chemical system. Competent editors and associates guarantee its scientific soundness. We wish *Protoplasma* all success.

AN INTRODUCTION TO BIOLOGY.
By Alfred C. Kinsey. *J. B. Lippincott Co.*
$1.68. 7¾ x 5¼; xiv + 558 *Philadelphia*

This new high school text for courses in general biology is especially noteworthy for its illustrations. While mainly garnered from existing sources the author has shown much originality and freshness of viewpoint in their assembling. We commend this book to the attention of teachers.

WERK UND WIRKUNG. *Eine Vortragsreise. (Moderne Biologie Heft 11.)*
By Prof. Dr. Hans Much. *Curt Kabitzsch*
7.20 marks 7½ x 5; 227 (paper) *Leipzig*

A series of lectures on various medical and biological-philosophical subjects given by the author, who is a professor at

Hamburg, in Budapest, Pecs, Agram, Barcelona, Madrid, and Davos. There is some interesting, and in part original, philosophical speculation in the book.

THEORETICAL BIOLOGY.
By J. von Uexküll. *Harcourt, Brace and Co.*
$5.50 8½ x 5½; xvi + 362 *New York*

It is a useful thing to have Baron von Uexküll's well-known treatise on theoretical biology translated into English. Biologically his position is vitalistic. Philosophically he derives from Kant, and ends in a position which has some points of resemblance to that of Whitehead. The value of the book is impaired by the absence of an index.

GOLDFISH CULTURE FOR AMATEURS. *How to Breed and Rear Goldfish in Aquaria and Ponds.*
By A. E. Hodge and Arthur Derham.
Frederick A. Stokes Co.
$1.50 5¼ x 7¾; xi + 103 *New York*

A thoroughly practicaly little treatise. The authors speak with the authority of first-hand-experience. It is entertaining to know that Epsom salts, that noble therapeutic agent, will cure goldfish of dyspepsia.

DIE EXISTENZGRÜNDE DER ZELLBILDUNG UND ZELLTEILUNG, DER VERERBUNG UND SEXUALITÄT. *Untersuchungen aus dem Gebiete der exakten Biologie.*
By Victor Schiffner. *Gustav Fischer*
7.50 marks *Jena*
6¼ x 9¼; v + 160 (paper)

A philosophical and metaphysical discussion of the data of biology, leading to a pronounced mechanistic position.

HUMAN BIOLOGY

THE UPPER PALAEOLITHIC AGE IN BRITAIN.
By D. A. E. Garrod.
Oxford University Press
$3.50 9 x 6; 211 *New York*

Miss Garrod has done a useful service in bringing together, with abundance of illustrations, all that is known regarding the presence of later paleolithic man in England. The work is honored by an introduction from the pen of M. l'Abbé Henri Breuil, whose pupil Miss Garrod was. The general conclusions are:

> We see this country as the Ultima Thule of Upper Palaeolithic Europe, a north-west cape, remote and inhospitable, bounded by the great ice-sheet under which Scotland and Ireland still lay buried. As we should expect, traces of human occupation are scarce, but we can make out the same general succession of industries as in other parts of western Europe, with certain gaps and certain variations.
>
> The most abundant remains are of the Aurignacian, and its Middle, or 'Typical,' phase as represented at Paviland, does not differ essentially from that of France and Belgium. In Upper Aurignacian levels, however, traces of a distinct local facies begin to appear in the shape of semi-geometrical forms, derivatives of the Gravette point, of which the most characteristic is an elongated trapeze (Paviland, Langwith, Creswell Crags). At the same time the proto-Solutrean point retouched on the bulbar face, which in France and Belgium occurs in the Font-Robert level, that is, at the very end of the Aurignacian, shows a special development.
>
> From this point onwards the English record grows obscure. As we have seen the increased cold of late Upper Palaeolithic times brought about a return of arctic conditions and industrial remains are rare.

NOTES ON DEMOCRACY.
By H. L. Mencken. *Alfred A. Knopf, Inc.*
$2.50 4¼ x 7½; v + 212 *New York*

U. S. A SECOND STUDY IN DEMOCRACY.
By H. E. Buchholtz. *Warwick and York, Inc.*
$2.20 7½ x 5; ix + 400 *Baltimore*

These two books cover the same ground in many respects. Taken together they furnish a devastating criticism of democracy. Mr. Buchholz demolishes it as practised, and Mr. Mencken both in practise and in theory. The latter stops at that point, and offers no guaranteed curative eye-water. Mr. Buchholz is bolder. He has a scheme to make democracies function better. But its efficacy is open to debate. His plan, from a detached viewpoint, has a suspicious likeness to the activities of the Anti-Saloon League. It seems to add one more brand of uplift to the already considerable number which annoy intelligent people. But perhaps it is along that pathway that evolution will choose to go. Mr. Mencken's book, beyond exhibiting his superb literary talents at their very best, is perhaps the most important contribution to the world's intellectual resources that he has yet made, not excepting *The American Language*. We welcome both books as valuable additions to the literature of human biology.

DIE MENSCHLICHEN SKELETRESTE AUS DER STEINZEIT DES WAUWILERSEES (LUZERN), *und ihre Stellung zu anderen anthropologischen Funden aus der Steinzeit.*

By Otto Schlaginhaufen. *Eugen Rentsch*
19 francs (Swiss) *Erlenbach-Zürich*
10¾ x 8; 278, 12 plates (paper)

A thorough metrical study of six finds of skeletal remains of prehistoric man, by the Director of the Anthropological Institute of the University of Zürich. The bones came from an old lake bed in *Kanton Luzern*. The material consists of (1) the skeleton of a woman about 30 years old and only 142.3 cm. in height. (2) A left femur. (3) A left os calcaneus. (4) A group of bones consisting of a left humerus, two defective pelvic blades, and a left femur. (5) A group of defective bones, including a skull cap. (6) A left parietal bone. It is all apparently neolithic. The volume is beautifully printed, contains an extensive bibliography, and a detailed index, and besides the text figures is illustrated with 12 superb photogravure plates. Altogether it is a substantial contribution to the technical literature of anthropology.

UNGARISCHE BIBLIOTHEK III. 2, *Bibliographia Hungariae. II. Geographica. Politico-oeconomica. Verzeichnis der 1861–1921 erschienenen, Ungarn betreffenden Schriften in nichtungarischer Sprache.*
Edited by Robert Gragger.
Walter de Gruyter und Co.
22 marks *Berlin and Leipzig*

Whatever biological interest this bibliography of literature about Hungarian geography and political economy may have derives from the fact that both subjects are looked at broadly, and there is a good deal of useful anthropological and statistical material noted.

GRUNDRISS DER ANTHROPOLOGIE.
By M. W. Hauschild. *Gebrüder Borntraeger*
10.50 gold marks *Berlin*
6½ x 10; viii + 235 (paper)

This book is only the first draft of what the author intended to be a much more extensive work. But his untimely death from malaria, contracted on a scientific expedition in Java, prevented the carrying out of the larger plan. The smaller work, as he left it, has been edited and prepared for the press by Professor Eugen Fischer. It is a treatise on physical anthropology, written from the viewpoint

of a trained anatomist. It does not deal systematically with the different races of men, but rather discusses the anatomy and biology of those human structures which are anthropologically important. It will interest and stimulate the general biologist more than most anthropological treatises.

THE HUMAN BODY.
By Marie Carmichael Stopes.
G. P. Putnam's Sons
$2.50 *New York*
5¼ x 8; viii + 268 + 7 plates

The versatile and accomplished Dr. Stopes has now written a little treatise on elementary human anatomy and physiology. The depth of its philosophy is indicated by the following account of the liver:

> The liver is one of the largest and heaviest organs in the body and its dark crimson texture is both soft and firm, the cells richly packed with food material and the whole interspersed with fine ramifications of the blood vessels. The old phrase: 'Is life worth living—All depends on the liver' has a great deal of truth in it in its purely materialistic sense, and those who are 'liverish,' whose liver and bile ducts do not act properly, suffer many of the minor inconveniences which go with indigestion and that uncomfortable feeling of swimminess in the head which precedes sickness.

There are six colored anatomical charts at the end, besides numerous text illustrations.

FROM TRIBE TO EMPIRE. *Social Organization among Primitives and in the Ancient East.*
By A. Moret and G. Davy. Alfred A. Knopf
$6.00 9⅛ x 6⅛; xxx + 371 *New York*

In this valuable contribution to the series *The History of Civilization* a historian and a sociologist collaborate in an attempt to trace the origin of political organization. In Egypt prior to 4000 B. C. the social organization was that of clans, which the authors find to be the first social organization among uncivilized peoples, rather than the family. About 3300 B. C. Menes founded the centralized monarchy and established the divine right of kings. The historical records of Egypt, Chaldea, and the Near East generally, enable the authors to trace the actual political development up to the firmly established empire. There is a bibliography, and an index.

PRÉHISTOIRE DE LA NORVÉGE.
By Haakon Shetelig.
Harvard University Press
$1.85 7¾ x 5¼; 280 (paper) *Cambridge*

An interesting, scholarly résumé of the results of archeological research in Norway, starting with the Old Stone Age and coming down to the Vikings. The book is beautifully printed, and illustrated with half-tone plates. There is a bibliography covering some eleven pages, and a detailed index.

OUR PREHISTORIC ANCESTORS. *The Story of Man's Evolution to the End of the Old Stone Age.*
By Dorothy Davison. Methuen and Co., Ltd.
7s. 6d. 5 x 7; xiv + 208 *London*

A popular and derivative account of early man, and his artefacts. The author adopts the views of Professor Elliot Smith on most controverted points. The book is extensively and rather attractively illustrated with line drawings. It is written in a pleasant, easily readable style.

MODERN SCIENCE AND PEOPLE'S HEALTH.
Edited by Benjamin C. Gruenberg. Contributing Authors: Charles R. Stockard, Hugh S. Taylor, Walter H. Eddy, William A. White, C. E. A. Winslow, Haven Emerson.
W. W. Norton and Co.
$2.50 5½ x 8½; vi + 250 *New York*

A high-toned contribution to the uplift. Dr. Gruenberg is a competent editor and has kept the amount of pious exhortation at a minimum and the well-written, sound science at a maximum. We congratulate him on producing so interesting a book, and driving so successfully a six-handed team of mettlesome scientists. The topics discussed, after Dr. Gruenberg's introductory essay about science in a democracy, are: Heredity and environment (Stockard); biological chemistry (Taylor); nutrition (Eddy); psychiatry (White); preventive medicine (Winslow); public health (Emerson).

ZOOLOGY

PROTOZOOLOGY. *A Manual for Medical Men, Veterinarians and Zoologists. Volumes I and II.*
By C. M. Wenyon. William Wood and Co.
$25 per set 9½ x 6½; xvi + 1563 *New York*

This is a monumental treatise. It aims to cover the whole field of protozoology, but designedly the free-living forms are discussed only briefly while the parasitic forms are treated in great detail, and with a wealth of illustrations. This book, taken with Calkins' *Biology of the Protozoa*, recently noticed in these pages, furnishes the zoologist with a thorough, critical, up-to-date résumé of the present state of knowledge of the biology of the protozoa. Wenyon's wide experience as a medical zoologist enables him to speak with first hand authority about the parasitic protozoa. But his viewpoint throughout holds fast to the sound anchor of general biology. He says:

> The student of the Protozoa which are pathogenic to man and domestic animals should have a sound knowledge of other parasitic Protozoa, and at least a good working knowledge of non-parasitic forms as well. Conversely, those who study free-living Protozoa should have a clear conception of the parasitic forms, for the extensive investigations of recent years have contributed so much to our knowledge that in many respects they are better known than their free-living relations, particularly as regards the completeness of their life-histories and the probable course of their evolution.

There is a very extensive bibliography, covering 98 pages of fine print, and a detailed index, covering nearly 50 pages. Altogether this is a substantial and welcome contribution to the literature of zoology.

THE NATURAL HISTORY OF ANTS.
From an Unpublished Manuscript in the Archives of the Academy of Sciences of Paris. By René Antoine Ferchault de Réaumur. Translated and Annotated by William Morton Wheeler. *Alfred A. Knopf*
$5.00 9½ x 6¼; xvii + 280 *New York*

Professor Wheeler has performed a very valuable service in bringing to light this memoir on ants by the great naturalist Réaumur and making it available for present day students. The manuscript has hitherto been lying unpublished in the archives of the Academy. In this beautifully printed volume the French text is given in full, prefaced by a biographical sketch of Réaumur and followed by Professor Wheeler's translation of it into English, and his extensive annotations. It is a superb piece of scholarly work, done in a manner that no one but Wheeler could achieve. All biologists will welcome it as an important contribution to the history of science.

ARISTOCRATS OF THE AIR.
By Capt. C. W. R. Knight, and a Preface by Viscount Grey of Fallodon.
Frederick A. Stokes Co.
$7.50 10 x 7½; xii + 166 *New York*

Capt. Knight is a keen ornithologist, who brings together in this book a series of superb photographs, illustrative particularly of the nesting habits of some of the rarer British birds of prey, along with an entertaining textual account of his observations of these birds. Interesting and valuable records are given of a family of Montagu's harriers, of a heronry, and of a shelduck's nest, among others. The value of the book from a zoological viewpoint would have been enhanced if scientific names had been given along with the common names. In the last chapter the author gives a brief account of the training of hawks for "the sport of kings"—falconry—at which he is an expert.

INSECTS OF WESTERN NORTH AMERICA. *A Manual and Textbook for Students in Colleges and Universities and a Handbook for County, State and Federal Entomologists and Agriculturists as well as for Foresters, Farmers, Gardeners, Travelers, and Lovers of Nature.*
By E. O. Essig. *The Macmillan Co.*
$10.00 6 x 9¼; xi + 1035 *New York*

This is a working manual of general entomology, abundantly and well illustrated. It will be widely useful as a reference work, since it will serve as a sort of colossal index to insects and the literature about them. Covering the whole field taxonomically as the book does, though limited geographically, descriptions are necessarily brief and keys go generally only to families. From an editorial point of view the tremendous amount of detailed work which the book represents has been extremely well done.

THE PLANT LICE OR APHIDIDAE OF GREAT BRITAIN. *Vol. I.*
By Fred V. Theobald. *Headley Bros.*
25 shillings *London*
8¾ x 5¾; ix + 372 (paper)

The first volume of a useful systematic monograph on the aphids, by the leading British authority on the group. The description of each form is followed by an account of what is known regarding its life history. The volume opens with a brief account of the external anatomy, and the general biology of aphids. The taxonomic portion is well illustrated with line drawings, which will be of material help in identification.

HOW INSECTS LIVE. (*An Elementary Entomology.*)
By Walter H. Wellhouse.
The Macmillan Co.
$5.00 5¾ x 8½; xv + 435 *New York*

This excellent elementary textbook by the associate professor of entomology at Iowa State College, after first describing briefly the essentials of insect anatomy and physiology, devotes a series of chapters to more detailed accounts of each order of insects. At the end of the book is a chapter on collecting with a key to the orders and families. The book is well illustrated and indexed, and gives brief bibliographical lists at the end of each chapter.

MAMMUTLEICHEN UND URWALDMENSCHEN IN NORDOST-SIBIRIEN.
By E. W. Pfizenmayer. *F. A. Brockhaus*
M. 14 6 x 9; 341 *Leipzig*

This is an interesting and well illustrated popular account of two expeditions, sent by the St. Petersburg Academy of Sciences in 1901–2 and 1908 to North

Siberia in search of mammoth remains. The sensation caused by the finding of the Beresowka specimen with skin and many soft parts intact and capable of histological study will be recalled. The long delay in the publication of the book is the result of a series of misfortunes which befell the author, in which the World War is included. But belated as it is the book is well worth reading. It contains a good many interesting anthropological and ethnological observations.

THE NEW NATURAL HISTORY. *Volumes II and III.*
By J. Arthur Thomson. G. P. Putnam's Sons
$6.00 each volume *New York*
8 x 10½; Vol. II, 383
Vol. III, xiv + 383

These two volumes maintain the high standard set in the first volume of this popular natural history, in respect of both text and illustrations. Professor Thomson has no present peer as a writer of popular biology. The chapter on "Life-Histories" in the second volume, and that on "Animals and Man" in the third are especialy worthy of praise. Father, even though he be a professional biologist, should not leave the reading of these chapters entirely to the children.

DIE ÖKOLOGIE DER BLATTMINIERENDEN INSEKTENLARVEN. (*Zoologische Bausteine, Band 1, Heft 2.*)
By Martin Hering. Gebrüder Borntraeger
18 marks 6½ x 10; 254 (paper) *Berlin*

This memoir includes the general biological portion of a comprehensive memoir on the leaf mining insects, which is projected by the author, who is a staff member of the Zoological Museum at Berlin. It is a thorough review of the literature on the ecological and general biological side, grounded upon the author's extensive personal researches in the field. There is a bibliography covering nineteen pages, an excellent index, and two plates. It is a valuable addition to the literature.

DIE TIERWELT DER NORD- UND OSTSEE. *Lieferung IV, Lieferung V.*
Edited by G. Grimpe and E. Wagler.
Akademische Verlagsgesellschaft M. B. H.
Mk. 18 8½ x 6 *Leipzig*
Mk. 8.80 Lieferung IV, 250 (paper)
Lieferung V, 116 (paper)

These two numbers in this monograph of the North Sea fauna, previous parts of which have been noticed in earlier numbers of THE QUARTERLY REVIEW OF BIOLOGY, cover the following subjects: Bryozoa, by E. Marcus; Fishes, general part, by H. M. Kyle and E. Ehrenbaum; Teleostei Physoclisti, by G. Duncker and Erna W. Mohr; Lamellibranchia, by F. Haas; and Copelata, by A. Bückmann. An excellent standard of quality is maintained throughout.

NOTES ON THE GAME BIRDS OF KENYA AND UGANDA. (*Including the Sand-Grouse, Pigeons, Snipe, Bustards, Geese, and Ducks.*)
By Sir Frederick J. Jackson.
Williams and Norgate, Ltd.
25 shillings 9 x 6; xv + 258 *London*

An interesting contribution to local ornithology covering systematically the groups and territory indicated by the title. The illustrations are in the form of colored plates reproduced by color printing from usually lithographic originals published elsewhere. The synonymy is given for each species, a brief description, and notes on habits and

habitats. Curiously enough, considering the character of the book, there is no index.

BRITISH SNAILS. *A Guide to the Non-Marine Gastropoda of Great Britain and Ireland Pliocene to Recent.*
By Arthur Erskine Ellis.
Oxford University Press
10 shillings *Oxford*
$7\frac{3}{4}$ x 5; 275 + 14 plates

A taxonomic manual of the land and freshwater molluscs of the British Isles. The more common species are figured on 14 photogravure plates. Brief notes on habits, habitat, and geographical distribution follow each diagnosis. The work is carefully done. It is a useful handbook for the field naturalist. In a good many cases the original description is quoted *verbatim*.

THE HOUSE-FLY. *Its Life History, Importance as a Disease Carrier, and Practical Measures for its Suppression. (Economic Series No. 1A.)*
By E. E. Austen.
British Museum (Natural History)
1 shilling $5\frac{1}{2}$ x $8\frac{1}{4}$; 68 (paper) *London*

The second edition of a well written and illustrated popular monograph on *Musca domestica*, in which special attention is given to means of suppression.

ZOOLOGIE IM GRUNDRISS.
By Walter Stempell. *Gebrüder Borntraeger*
10,50 marks *Berlin*
Lieferung 4, $7\frac{1}{8}$ x $10\frac{1}{2}$; xx + 212 (paper)

Brings to a close an excellent general zoology, of which the earlier parts have been noticed in previous numbers of THE QUARTERLY REVIEW OF BIOLOGY.

A GENERAL CONSIDERATION OF SNAKE POISONING AND OBSERVATIONS ON NEOTROPICAL PIT-VIPERS. *Contributions from the Harvard Institute for Tropical Biology and Medicine, II.*
By Afranio do Amaral.
Harvard University Press
$2.50 *Cambridge*
$10\frac{1}{2}$ x $7\frac{1}{2}$; 64 + 16 plates

The first 18 pages of this monograph deal generally with snake poisoning, while the remainder of the book concerns itself with taxonomic matters pertaining to various species of *Bothrops*. The work was done under the direction of Dr. Thomas Barbour, which ensures its soundness. It is illustrated with 16 plates, mostly colored.

INSTRUCTIONS FOR COLLECTORS: *No. 7—Blood-Sucking Flies, Ticks, etc.*
By E. E. Austen.
British Museum (Natural History)
Sixpence $5\frac{1}{2}$ x $8\frac{1}{4}$; 28 (paper) *London*

Brief descriptions of the appearance and habits of blood-sucking flies other than mosquitoes, with detailed directions for collecting and forwarding such animals to England. There is also a section on collecting fleas, bugs, lice, ticks and smaller mites.

A SYNOPSIS OF THE FAMILIES AND GENERA OF NEMATODA.
By H. A. Baylis and R. Daubney.
British Museum (Natural History)
10s. 6d. *London*
$5\frac{3}{4}$ x $8\frac{3}{4}$; xxxvi + 277

An excellent taxonomic monograph including all the genera of nematodes described prior to the end of the year 1923. The freeliving and parasitic nematodes are

here united in a common system of classification. It seems a pity that there are no illustrations whatever.

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Lieferung 199.* Containing following articles: *Methoden zur Erforschung des Vogelzuges*, by J. Thienemann; *Methoden zur Behandlung der Atemphysiologie der Insekten*, by Albert Koch; *Die Verfahren zur Erforschung der Tierfluges*, by Oskar Prochnow.

Urban und Schwarzenberg

7.50 marks 7 x 10; 172 (paper) *Berlin*

The first paper in this number of the Abderhalden handbook is brief, and deals with methods of bird banding, etc., in connection with the study of migration. The second paper on the respiration of insects is an important and valuable one. So also is the last on animal flight.

A LABORATORY MANUAL FOR ELEMENTARY ZOOLOGY.

By Libbie Henrietta Hyman.

University of Chicago Press

$2.50 10 x 7; xviii + 182 *Chicago*

The second edition of the laboratory manual used for a number of years in the beginning zoology course at the University of Chicago. The directions are very detailed, a procedure which is ably defended by the author in the preface.

SYNONYMY OF THE BRITISH NON-MARINE MOLLUSCA. (*Recent and Post-Tertiary*) *Compiled and annotated by A. S. Kennard and B. B. Woodward.*

British Museum (Natural History)

£1 5½ x 8¾; xxiv + 447 *London*

A detailed, critical compilation of the synonomy of the British species of land and fresh water molluscs, of interest and usefulness to taxonomic malacologists.

BOTANY

WILHELM HOFMEISTER. *The Work and Life of a Nineteenth Century Botanist. Ray Society Volume No. 111.*

By K. von Goebel. Translated into English by H. M. Bower and edited botanically by F. O. Bower. *Dulau and Co., Ltd.*

12s. 6d. 8¾ x 5½; xi + 202 *London*

For many years the Ray Society has performed the useful service of issuing annually one or two important zoological or botanical monographs, usually taxonomic in character, but with the occasional interpolation of an important biographical memoir. This memoir of Hofmeister by Goebel, his last surviving pupil, is a valuable contribution to the history of science. Goebel devotes this part of the book to a critical exposition of the significance to the development of biology of his master's work. This is followed by an appendix of some fifty odd pages, in which is given a simple and rather charming account of Hofmeister's life, written by his daughter, Frau Constanze Ganzenmüller.

DIE PFLANZENAREALE. *Sammlung kartographischer Darstellungen von Verbreitungsbezirken der lebenden und fossilen Pflanzen-Familien, -Gattungen und -Arten. 1 Reihe, Heft 1, und Heft 2.*

Edited by E. Hannig, H. Winkler, Ludwig Diels and G. Samuelsson. *Gustav Fischer*

7.50 marks each 13 x 12 *Jena*

Heft 1, iii + 14 + 10 maps (paper)

Heft 2, 16 + 10 maps (paper)

These first two numbers of an undertaking, which if realized will be extremely useful to all students of evolution, are excellently done. The maps are well printed and the indications of distributional areas are beautifully clear. Their critical accuracy is guaranteed by the fact that each group is handled by a specialist. The authors and groups mapped in these two numbers are as follows: A. Engler, *Saxifraga* I; F. Pax, *Acer* I; L. Diels, *Casuarina;* F. Vierhapper, *Soldanella;* M. Rikli, *Pinus pinea;* E. Hannig, *Genista anglica;* Hub. Winkler, Musaceae; F. Pax, *Sapium;* J. Mattfeld, European species of *Abies;* L. Lämmermayr, *Fagus silvatica* and *Fagus orientalis;* E. Hultén, *Pinus pumila* and *Hierochloë pauciflora.*

GENERAL BOTANY. *With Special Reference to its Economic Aspects.*
By C. Stuart Gager.
P. Blakiston's Son and Co.
$4.00 8½ x 5¾; xvi + 1056 *Philadelphia*

This is an excellent textbook of general botany of college and university grade. Written out of wide knowledge and sound judgment it seems likely to have a large field of usefulness. Three chapters on plant genetics are contributed by Dr. O. E. White. There are 689 illustrations, a detailed index, and abundant documentation of the literature. Altogether it is a first-rate piece of textbook writing.

MANUAL OF PLANT DISEASES.
By Frederick DeF. Heald.
McGraw-Hill Book Co.
$7.00 6 x 9; xiii + 891 *New York*

A comprehensive general text book of plant pathology, which attempts to cover the whole field, including plant diseases due to direct environmental injuries, as well as those caused by bacteria, fungi, and filterable viruses. The discussion of each disease is followed by a brief bibliography of the important literature on it. The book is well illustrated and indexed. It is a valuable addition to the text book literature.

PFLANZEN-PHYSIOLOGISCHE STUDIEN.
By Rose Stoppel. *Gustav Fischer*
7.50 marks 6½ x 9½; 165 (paper) *Jena*

The purpose of the author of this volume, who is a *Privatdozent* at the University of Hamburg, is to act as a *liaison* officer between the plant physiologists, on the one hand, and the animal physiologists and medical men, on the other hand. The book gives a brief *résumé* of the present status of plant physiology, under the following heads: Respiration, fermentation, narcosis, photodynamic sensitization, chlorophyll, assimilation of carbon dioxide, transpiration, water absorption and conduction, growth, geotropism, phototropism, conduction of stimuli, periodic phenomena. The book is well written, and is interesting, but suffers from the lack of an index, and of illustrations.

A MONOGRAPH OF THE BRITISH LICHENS. (*A Descriptive Catalogue of the Species in the Department of Botany, British Museum.*) *Part II.*
By Annie L. Smith.
British Museum (Natural History)
20 shillings *London*
8¾ x 6; ix + 447; 63 plates

A second, revised, edition of Part II of this standard handbook on the taxonomy of the lichens. The plates are excellent for purposes of identification.

CARGOES AND HARVESTS.
By Donald C. Peattie. *D. Appleton and Co.*
$2.50 8¼ x 5½; 311 *New York*

An interesting popular account, well written and quaintly illustrated, of the history of agricultural exploration, and of the development of plant products for the use of man. There are useful brief bibliographies following each chapter, and a detailed index.

FIELD MANUAL OF TREES. *Including Southern Canada and the Northern United States to the Southern Boundary of Virginia, Kentucky and Missouri Westward to the Limits of the Prairie. Third Edition.*
By John C. Schaffner. *R. G. Adams and Co.*
$1.50 7 x 4½; 154 *Columbus, O.*

The third edition of a useful little determinative manual for the trees of the Northern United States and Southern Canada, by the professor of botany at Ohio State University.

PLANTS AS INVENTORS.
By R. H. Francé.
Simpkin, Marshall and Co.
1 shilling 7¾ x 5½; 63 *London*

An entertaining little popular book showing how the essential elements of man-invented mechanisms are found duplicated in living things, particularly plants.

UNTERSUCHUNGEN ÜBER MARCHANTIACEEN. *Botanische Abhandlungen, Heft 10.*
By Dr. E. Bergdolt. *Gustav Fischer*
4 marks 6½ x 9½; 86 (paper) *Jena*

This monograph, in the series edited by Professor Goebel, is a contribution to the morphology, embryology and cytology of the liverworts. There is a bibliography of sixty-six titles.

MORPHOLOGY

ANATOMY OF THE WOOD RAT. *Comparative Anatomy of the Subgenera of the American Wood Rat (Genus Neotoma). Monographs of the American Society of Mammalogists. No. 1.*
By A. Brazier Howell.
The Williams & Wilkins Co.
$5.00 6 x 9; x + 225 *Baltimore*

This is a thorough descriptive treatise covering the external morphology, osteology, myology and visceral anatomy (alimentary tract and urogenital system) of the sub-genera of wild wood rats, *Neotoma*, *Homodontomys* and *Teonema*. Besides being a contribution to comparative mammalian anatomy, it will be found useful by all the hosts of laboratory workers who use the white rat as material. The book is well, though not lavishly, illustrated. The last chapter is devoted to a discussion of muscle action and limb motion—functional anatomy of the muscular system in short. There is a brief bibliography and an index.

CARL GEGENBAUR. *Rede zum Gedächtnis seines hundertsten Geburtsjahres, gehalten im Hörsaal der Anatomischen Anstalt in Heidelberg am 19. April, 1926.*
By Friedrich Maurer. *Gustav Fischer*
1.20 marks 6½ x 9½; 18 (paper) *Jena*

A brief review of the life of the great comparative anatomist, with some portraits.

PHYSIOLOGY

L'EXPLORATION FONCTIONNELLE DU FOIE ET L'INSUFFISANCE HÉPATIQUE.
By Noël Fiessinger and Henry Walter.
Masson et Cie.
$1.20 6¼ x 9¼; 387 (paper) *Paris*

This book will interest those specialists in internal medicine who are trying today to find some method of estimating the function of the liver. Unfortunately for the authors, our knowledge of the physiology of the liver and of bile pigment formation has grown so rapidly in the last two years, since this book was written, that it can hardly serve as a text for the man who is on the firing line of research. It will, however, be a help to those students who wish to know more about the history and development of the subject, particularly in France. Over a third of the book deals with the clinical picture of hepatic insufficiency and methods of treatment. There is no index.

THE RESPIRATORY FUNCTION OF THE BLOOD. *Part I. Lessons from High Altitudes.*
By Joseph Barcroft. *The Macmillan Co.*
$5.00 9¾ x 6¾; x + 207 *New York*

The deserved success of Professor Barcroft's *The Respiratory Function of the Blood* has necessitated the preparation of a second edition. The advance in the subject has meanwhile been so great as to make it desirable to split the book into a series of volumes, of which this is the first. It deals with the results of the author's various physiological expeditions to high mountain localities. The book is by way of being a classic.

BEZIEHUNGEN ZWISCHEN CHEMISCHER KONSTITUTION UND PHYSIOLOGISCHER WIRKUNG.
By E. Rost. *Urban und Schwarzenberg*
1.20 marks 7 x 10; 24 (paper) *Berlin*

The correlation between the chemical composition of drugs and their physiological actions constitutes one of the prime problems of pharmacology. The present contribution, which is a reprint from the *Handbuch der praktischen und wissenschaftlichen Pharmazie* edited by Thoms, is a brief review of the present state of knowledge of this field of investigation. There is a fairly extensive bibliography.

BIOCHEMISTRY

SURFACE EQUILIBRIA OF BIOLOGICAL AND ORGANIC COLLOIDS.
By P. Lecomte du Noüy.
Chemical Catalog Co.
$4.50 6 x 9; 212 *New York*

This contribution to the American Chemical Society's Monograph Series is of interest to both biologists and students of molecular physics. It recounts in detail the distinguished author's own researches at the Rockefeller Institute. Starting from the theoretical inferences as to equilibrium which can be made from the Gibbs-Thomson thermodynamic formula Du Noüy demonstrates experimentally "that the proteins and other substances which constitute living matter have a tendency to concentrate at interfaces. They even carry with them part of the salts which, if alone in a solution, would show the opposite tendency. The precipitation or the coagulation of proteins may in certain cases be facilitated by

this accumulation of the molecules. But a still more important conclusion can be drawn: i.e., that the most probable configuration of equilibrium of such a system is the cell form."

ENZYMES. *Properties, Distribution, Methods and Applications.*
By Selman A. Waksman and Wilburt C. Davison. *The Williams & Wilkins Co.*
$5.50 6 x 9; xii + 364 *Baltimore*

This useful digest of the literature on enzymes is divided into four main parts, which deal successively with the properties of enzymes, their distribution among living organisms, methods of preparing and studying them, and their practical application in the arts and industries. There is a biblography of over thirteen hundred titles, and an index. The book will be a valuable work of reference in every biological laboratory.

LE pH INTÉRIEUR CELLULAIRE.
By Paul Reiss.
Les Presses Universitaires de France
50 cents 10 x 6½; 135 (paper) *Paris*

A detailed critical review of the literature regarding the determination of the hydrogen ion concentration of the contents of the individual cell. It will be a useful work of reference for all cytologists and experimental biologists generally. There is a bibliography covering some seven pages. The author has himself done considerable original work in the field.

PHYSIKALISCHE CHEMIE DER ZELLE UND DER GEWEBE.
By Rudolf Höber. *Wilhelm Engelmann*
42 marks 6½ x 9½; xvi + 955 *Leipzig*

The sixth edition of a standard treatise, which has now indeed become a classic in the field. This edition is considerably revised from the fifth, of two years ago, the separation between pure and applied physical chemistry particularly being made more sharp.

HYDROGEN ION CONCENTRATION. *Its Significance in the Biological Sciences and Methods for its Determination. Vol. I. Principles of the Theory.*
By Leonor Michaelis. Translated from the German by Wm. A. Perlzweig.
The Williams & Wilkins Co.
$5.00 9 x 6; xiv + 299 *Baltimore*

It has been necessary in the second edition of Dr. Michaelis' well known treatise to divide it into two volumes. The first, now before us, deals with the fundamental principles of the theory of physical chemistry, while the second will discuss methods of measuring hydrogen ion concentration, and applications. The book has become one of the classic texts of modern biology and medicine.

LEHRBUCH DER PHYSIOLOGISCHEN UND PATHOLOGISCHEN CHEMIE. *In 75 Vorlesungen. Für Studierende, Ärzte, Biologen und Chemiker. I Band: Organchemie. II Lieferung: Muskel- und Nervensubstanz, Stütz- und Gerüstsubstanzen, Leber, Niere und Lymphatische Organe. Vorlesung XVII bis XXIX.*
By Prof. Dr. Otto Fürth. *F. C. W. Vogel*
15 marks *Leipzig*
10 x 7; v + 207 (paper)

This second part of the second edition of Professor Fürth's textbook of physiological and pathological chemistry maintains the high standard of quality set in the first part, which was reviewed in an

earlier number of THE QUARTERLY REVIEW OF BIOLOGY.

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Lieferung 202.* Containing following articles: *Flockung von Kolloiden* and *Die Bestimmung des isoelektrischen Punktes*, by Ernst Bloch; *Methoden zur Bestimmung der Adsorption*, by Andor Fodor; *Methoden der biologischen Dispersoidanalyse*, by Friedrich-Vincenz v. Hahn; *Ermittlung der Struktur von kolloiden Mizellen*, by Rassa Riwlin; *Ultrafiltration und Elektro-Ultrafiltration*, by Heinrich Bechhold.

Urban und Schwarzenberg

9.30 marks 7 x 10; 210 (paper) *Berlin*

This part of the Abderhalden handbook concerns itself with various aspects of the physics and chemistry of colloids, a subject just now greatly to the fore in various fields of biology.

SEX

SEX AND THE YOUNG.

By Marie Carmichael Stopes.

G. P. Putnam's Sons

$2.00 5¼ x 8; 248 *New York*

A wide-ranging discussion of the problems presented in the instruction of children in sex matters. Much of the advice given is excellent. The point of view underlying the whole book is the prevalent one that the child ought to be protected, guided, instructed and perhaps even coddled by older persons, instead of being left to develop his or her own knowledge of sex in his or her own way. Perhaps this is true. But it involves a curious point of logic. Of the arguments advanced by Stopes (and many others) for telling children all about sex early and often, *are* true, how does it come about that all, or at the very least nine-tenths, of the generation to which we belong and those to which our fathers and grandfathers belonged, were not physically and morally wrecked for the lack of these biologically proper parental precepts about sex, which we and our forebears wholly missed? It is a great puzzle. Perhaps all these lives *were* ruined and we never knew it.

PROBLEMS OF HUMAN REPRODUCTION.

By Paul Popenoe.

The Williams & Wilkins Co.

$2.50 7½ x 5; ix + 218 *Baltimore*

Some years ago there was a good deal of talk about the dreadful consequences of the assumed widespread and complete ignorance of the young about sex and reproduction. At the present moment there appears something like one book a week which tells all. Mr. Popenoe has lately been specializing in this field, with a flux of mingled physiology and moral precepts. The present volume seems to us a little better than some other "sex" books, perhaps because it contains more physiology and less preaching.

BIOMETRY

DIE INTRAINDIVIDUELLE FLUKTUIERENDE VARIABILITÄT. *Eine Untersuchung über die Abänderung des Pflanzenindividuums und die Periodizität der Lebenserscheinungen. Botanische Abhandlungen, Heft 9.*

By E. Dennert. *Gustav Fischer*

7 marks 9¾ x 6½; 149 (paper) *Jena*

An interesting biometrical study of intra-individual variation of the parts,

mostly leaves, of single plants. It is chiefly valuable for the data it presents. The biometric technique employed is very simple and scarcely adequate to any really penetrating analysis of the problem.

PSYCHOLOGY AND BEHAVIOR

THE INFLUENCE OF NURTURE UPON NATIVE DIFFERENCES.

By Truman Lee Kelley. *The Macmillan Co.*
$1.40 5 x 7¼; vii + 49 *New York*

An ingenious, if perhaps not entirely convincing, attempt to measure separately the effect of inheritance and of learning in the determination of the differences among individuals in respect of certain mental characteristics and abilities. The argument and technique are statistical in character and are stated very obscurely. The sort of final conclusion reached is indicated in the following statement, which however is to be understood as applying only to the abilities mentioned: "We may therefore conclude that approximately 97 per cent of the adult difference between Arithmetic, Reasoning and Spelling abilities is to be attributed to original nature. Thus nurture has in the long run negligibly influenced the native idiosyncrasies in abilities in these two fields."

THE POSSIBILITY OF METAPHYSICS.

A Course of Four Lectures Delivered Before the University of London in March, 1924.
By Hans Driesch. *The Faith Press, Ltd.*
2s. 6d. 7¼ x 5; 63 *London*

A contribution to theology by the distinguished professor of philosophy at Leipzig, and erstwhile zoologist. He has travelled a long way from sea-urchin eggs. It seems unlikely that biologists will be either thrilled or helped greatly by this treatise. Driesch states categorically (p. 58) that he is convinced that telepathy, mind-reading, clairvoyance, telekinesis, materialization and perhaps also premonition, *are facts*, and that they are more satisfactorily explained by the spiritualistic hypothesis than any other.

L'ART ET LA RELIGION DES HOMMES FOSSILES.

By G. H. Luquet. *Masson et Cie.*
$1.04 6½ x 10; 231 (paper) *Paris*

The position taken in this extremely well illustrated and printed treatise is the one widely prevailing at the present time, to the effect that the art of the prehistoric cave-dwellers found its basic motivation in religious and magical considerations. This may be true, but the evidence in its favor so far produced by anyone, including M. Luquet, seems to us to fall short of demonstrating this thesis. But objectively the book is fascinatingly interesting.

DE OMNIBUS REBUS ET QUIBUSDEM ALIIS

SCIENCE OF LOVE WITH KEY TO IMMORTALITY.

By Ida Mingle.
School of Liveable Christianity
$5.00 5½ x 8½; 1118 *Chicago*

The title of this book is informative in more senses than one. It will be recalled that the phraseology of this title is extraordinarily suggestive of that of another book, also bound in black cloth. Perhaps still another religion is being born in our midst, and perhaps this book with its

thirty-six lines (numbered) to its eleven hundred odd pages is its epic.

We can advise no one either to buy it or to read it through. But dipping in it yields entertainment. Like most religions it has a lot of sex in it. Indeed there seems to be overtly more of it in this treatise than in any other holy writings we are able at the moment to recall. The author is said to be "conspicuous for her capacity to explain all activities of life without condemnation, thereby becoming a fit instrument to write a book of such stupendous significance."

The book starts off, as it should, with evolution:

> The suggestion that man was once a monkey or some other form of anthropoid makes interesting delusion, but not good sense nor science. Man-like creatures are the result of the mixing of the animal mankind with the he and she monsters of the lower animal kingdom. Records of evolution show that when the Third Root race was forming and what is called man was only a huge animal, not yet directly controlled by the spirit of the Gods, a cross took place between the lower and the higher forms of these animal creatures, thereby crossing in the blood stream the spirit of the man-creature with the spirit of the beast. This cross was a sexual cohabitation and in a later cycle was repeated at the introduction of the white race into the regions of Australia, this cross producing the sterility and ultimate extinction of the Bushmen.

This, we submit, is getting away to a good start. It would be, like Artemus Ward's bear, amusing and instructive, to develop some other of the high spots of the book with quotations. But the QUARTERLY REVIEW OF BIOLOGY is a family magazine, and furthermore its financial position will not warrant the risk of having a number barred from the mails. Were it not for these inhibitory considerations the chapters on Generative Marriage; Material and Spiritual Marriage; Man-Woman; Soul-Mates; Divorcement; The Seed of the Woman; Christ, Jesus Christ, and Jesus; etc., etc., could yield up one of the most curious messes of nonsense that our readers have ever met. Under the circumstances the best we can do is to offer to let anyone who will come to Baltimore read the office copy for one hour without charge. That will be enough.

BLOOD AND SUPERMAN.
By H. Valentine Knaggs.
The C. W. Daniel Co.
3s. 6d. 7¼ x 4¾; 92 *London*

Mr. Knaggs is a prolific writer on various subjects from onions to rheumatism. But this book is perhaps his masterpiece. He says at the start that its subject-matter is "to a large extent based on Theosophical teaching." Unfortunately we cannot spare the space to detail all the wonderful conclusions reached. One sample will have to do. "Blood, by reason of its odorous principles which pass along the nerves, is the source from which our intellect is built up by the mind. There are three factors which build intellect: (1) The free mixing of blood that takes place among the more progressive races of mankind; (2) the stimulating diet—e.g. meat, sugar, alcohol, drug beverages and tobacco—to which Western nations are addicted; and (3) the intense thirst for wealth and enjoyment which is a feature of our modern life. Ancient man was *clairvoyant, but devoid of intellect.*"

REGINALD'S FATHER

Our new office boy, Reginald, comes of good sound New England stock, with traditions of reading and general interest in intellectual matters back of him. Furthermore there is a transparent and vigorous sincerity and honesty in the blood which is as rare as it is appealing

in our over-urbanized and over-sophisticated civilization.

Testimony to these qualities is borne by the following letter, which Reginald's father sent in the other day.

To the Editor,
Dear Friend:

We have seen much in the newspapers lately about evolution. Not knowing much about it I bought and read, after seeing the account of it in the QUARTERLY REVIEW OF BIOLOGY last year, a copy of a new and up-to-date book called "The Ways of Life" by Professor Lull of Yale College. I found it pleasant reading, and very informing to a plain farmer like myself, who has had plenty of chance to observe Nature, though but little to read books.

The Professor records many of the wonders of Nature in this book. I was especially struck by the following sentence on p. 65: "In certain of the New Brunswick rivers the capelin run at times in such great numbers as to fill the stream from bank to bank, and in such relic seas as the Caspian, where the number of kinds of animals are few, but of individuals very large, the cessation of fishing for a single day is said to make an appreciable increase in their numbers."

Now I never have myself fished in the Caspian Sea, but a cousin of my wife has a farm on the Lallegosh River in New Brunswick, and has many times seen just such runs as the Professor describes. He tells often of the run of the spring of 1917, when the fish were so packed in the river bed opposite his farm that all the water in the river was forced out and overflowed his cornfield to a depth of three feet. In fact the overflow extended clear to the house, and filled the cellar up to the second step from the top of the cellar stairs. The fish in the river were a sight which attracted all the farmers from the countryside, and a good many from the cities, even as far as St. John. Unable to swim, because they had crowded all of the water out of the river bed, the fish stayed packed solidly together headed upstream for eleven days. Then a sharp shower fell, beginning about half past two in the afternoon. Owing to the fact that the rubbish left by the visitors, who had come to see this curious sight, and picnicked along the shore, had raised the bank for about one foot for a distance of nearly two miles, this shower provided enough water to enable the fish to move on.

I am glad to be able on the unimpeachable testimony of a relative of the family (who is a professing brother of the Adventist faith) to confirm the Professor's account. In a way I can offer direct testimony of my own, because I furnished the seed to replant the cornfield. Furthermore it is nice on our side to have the Professor's confirmation, because some people have refused to believe my wife's cousin when he has told this truly marvelous tale.

It seems to me that some of those fish of the Caspian Sea, of which the Professor tells, ought to be imported into this country. Any fish that will perform in this manner described would be a valuable addition to our resources. The nice adjustment of the females of this species, which leads them to lay eggs all the year round, and in just sufficient numbers to replace the daily catch, is certainly one of the most beautiful adaptations which evolution has produced.

Yours truly,

NICODEMUS A. SKILLINGS,
R. F. D. #2
West Brown
Maine.

Reginald adds that the seed corn furnished by his father was a selected strain of Longfellow Flint, which he himself got originally from the Maine Agricultural College at Orono while attending Farmers Week.